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EDITORIAL.

ITEMS AND NOVELTIES.

The Dubuque Bridge—Official Report of the Test.—We are indebted for a copy of the following report to Walter Katte Esq., Engineer of the Keystone Bridge Company:

Having been invited by the Chief Engineer and other officers of the Dunleith and Dubuque Bridge Company to witness the test examination of their Railroad Bridge, just completed, over the Mississippi river, at Dubuque, we attended for that purpose, on the morning of Tuesday, December 29.

Col. R. B. Mason, Chief Engineer, assisted by J. E. Ainsworth, resident Engineer, had previously arranged five first-class locomotives, with the tenders, supplied with their full complement of fuel and water, making a gross weight, as estimated by Mr. J. C.

Jacobs, Superintendent, of about 246 tons. This train was run over the whole length of the bridge both ways, and was also allowed to rest some time upon single spans, being a little more than the length of the longer spans, which are 250 feet, and, as nearly as may be, one ton per lineal foot of the span.

With the above load the greatest deflection in the middle of the span was 0·138 of a foot, or about one inch and twenty-one thirty-seconds. On removing the load the span resumed its height at the centre, within one thirty-second of an inch.

The Draw Span, on its western half, which is 180 feet long from the pivot, was subjected to as much of the weight as it would contain on that length, or about one ton per foot lineal, and showed a deflection of 0·066, or about three-fourths of an inch. On removing the load the span resumed its height within 0·014 of a foot, or about one-sixth of an inch.

Applying a similar test of about one ton to the foot lineal of the span of 225 feet on the sixth span, the deflection in the middle of the span proved to be 0·128 of a foot, or one seventeen thirty seconds inches. On removing the load it assumed its original height, within 0·005 of a foot, or one-sixteenth of an inch. A similar test of the seventh span showed a deflection of 0·127 of a foot, or one and one-half inches. On removing the load it rose to its original height, within one hundreth of a foot—less than one-eighth of an inch. The span was then tested with an engine and one passenger car, and showed a deflection of 0·06 of a foot, or three-fourths of an inch. We also passed over the bridge with an engine drawing its usual passenger train, making a general examination of the entire structure.

All our examinations have satisfied us that the bridge is an excellent and safe structure, admirably adapted to the purpose for which it is designed: namely, to connect the railroads of Iowa and the Union Pacific Railroad with the railroads running through Illinois to Chicago and the eastern cities.

We learn that this bridge has been erected by a Company composed chiefly of residents of the city of Dubuque. The following are the names of the officers :—Hon. W. B. Allison, President; Wm. E. Massey, Secretary; Platt Smith, H. E. Stout, J. Tucker, and R. B. Mason, Directors; R. B. Mason, Chief Engineer; J. E. Ainsworth, Resident Engineer.

The sub-structure was in the hands of Reynolds, Salpaugh & Co. of Rock Island, who have fulfilled their contract in a very short

time, entirely to the satisfaction of the Company, and deserve great credit for their energy and skill.

The stone for the piers was brought by rail from Joliet and Lamont, Illinois.

The iron superstructure was built at Pittsburgh, Pa., by the Keystone Bridge Company, and put up by them in about eight months, under the supervision of Walter Katte, Esq., their Engineer.

It presents altogether one of the most remarkable engineering performances of the age. Eleven months ago the iron that now constitutes one of the finest bridge superstructures in the country existed only in the shape of the natural ore, or pig iron. Since that time it has been manipulated and erected as it now spans the majestic Mississippi.

The bridge is built from the designs of J. H. Linville, Civil Engineer, President of the Keystone Bridge Building Company. It consists of seven spans, two of 250 feet, four of 225 feet, and one of 360 feet, resting on the pivot pier, leaving the opening 160 feet wide on each side of the pier for river navigation. The total length of the bridge is 1760 feet, or exactly one-third of a mile. The truss is 28 feet high and 16 feet wide in the clear. The piers are of stone, founded on piles—about 180 piles to each pier, driven to an average depth of 27½ feet, cut off at about 13 feet below low water.

The eastern approach of the bridge is made by a tunnel, cut through tbe rock of the bluff, 835 feet long, 22 feet high, and 18 feet wide, the rock being solid and requiring no arching. The approach on the west side is made on strongly-built trestle-work, 3200 feet long. The distance from the depot, on the Iowa side, to the eastern end of the bridge, laid by the Bridge Company, is 6902 feet. There are 4772 cubic yards of masonry in the piers and abutments. In the superstructure there are 2,405,000 pounds of wrought iron, and 462,800 pounds of cast iron.

The lower chord of the superstructure is 31½ feet above low water, and ten feet above the highest known flood—the range of the water at this point being 21 feet.

In concluding this hastily prepared report, we beg leave to tender our acknowledgments to the officers of the Illinois Central and Northwestern Railroad Companies, for the courtesies extended by them, and to the officers of the Bridge Company, and the citi-

zens of Dubuque, for their numerous kind attentions during our visit to their beautiful city.

W. MILNOR ROBERTS,
Chief Engineer St. Louis and Illinois Bridge.
E. B. TALCOTT,
Civil Engineer.
E. H. JOHNSON,
Chief Engineer C. R. I. and P. Railroad.
JOHN E. BLUNT,
Engineer Galena Div. C. and N. W. Railway.
A. S. VAN MEENAN,
Engineer Wisconsin Div. C. and N. W. Railway.
GEO. W. MORGAN,
Engineer W. Div. P. F. W. and C. Railway.

Concrete Bridge.—The tests applied to the experimental bridge of concrete, set in cement, erected over that branch of the Metropolitan District Railway which forms one of the junctions between the circular line and the West London Extension, prove conclusively the reliable character of concrete exposed to compressive strains. The structure experimented upon spans the open cutting between Gloucester-road Station and Earle's Court road. It is a flat arch of 75 feet span and 7 feet 6 inches rise in the centre, where the concrete is 3 feet 6 inches in thickness, increasing towards the haunches, which abut upon the concrete skewbacks. The material of which the bridge is made is formed of gravel and Portland cement, blended in the proportions of six to one, carefully laid in mass upon close boarding set upon the centering, and enclosed at the sides.

In testing the bridge, rails were laid upon sleepers over the arch, which brought a load of $\frac{2}{7}\frac{3}{5}$ths of a ton per foot run upon the structure. Seven trucks, weighing, together with their loads, 49 tons, were formed into a train, having a wheel base of 57 feet; hence the rolling load amounted to $\frac{4}{5}\frac{2}{7}$ths of a ton per foot run. The deflection produced by the passage to and fro of this train four times, was noted upon a standard, cemented to the side of the arch, at a distance of one-third the span from the abutments. When one side of the bridge was loaded, the extreme rise of the branch on the opposite side was about $\frac{1}{1}\frac{1}{6}$th of an inch, which was produced by a maximum strain of 10 tons 14 cwt. per square foot.

At a subsequent trial, a mass of gravel, 10 feet wide and 3 feet thick at the crown, and 6 feet deep at the haunches, was laid over

the bridge, and upon this, ballast was placed the permanent way. After an interval of a few days, the trucks, loaded as before, were passed over the bridge, at first in pairs, and finally all together. In this test the strain upon the concrete was as follows:

The weight of the arch, as before	7 tons 17 cwt.
170 tons of ballast	4 tons 8 cwt.
Strain per square foot from dead load	12 tons 5 cwt.
Strain per square foot from passing load	2 tons 17 cwt.
Total strain per foot	15 tons 2 cwt.

After repeated transit, the load was left upon the bridge all night, and the arch, upon examination, showed no signs of failure or distress under the severe strains to which it had been exposed.

From these trials it is fair to assume, that a thoroughly well-constructed arch of concrete is absolutely stronger than a similar one of brick; but in practice the danger arises that it would be difficult to ensure so high a quality of concrete as that employed in the present instance, and the proper supervision of the contractor's work by the engineer would be almost impossible in structures of this material, whilst the inspection of brick work is an easy matter.

The utter uselessness of inferior concrete was shown by the failure of the bridge which was previously erected on the site of the present one, which yielded under its own load when the centres were struck.

The Cincinnati and Newport Bridge.—All preliminary arrangements and work has now been begun upon this bridge, which is to connect Butler Street, in Cincinnati, with Saratoga Street, in Newport, Kentucky.

The stone work of all the piers is to be of the best limestone, up to the line of high water, and freestone above that, excepting the two piers of the middle, or long span, which will be entirely limestone. Much of the stone for the piers has already been quarried; George A. Smith, of Cincinnati, has the contract for the stone work.

The bridge proper will be of the best wrought iron, in lower and upper chords, up-rights, braces, &c. No timber will be used save in the flooring. The Keystone Bridge Company has the contract for the bridge proper, which will be constructed after the popular and very safe patent of Linville & Piper, now in quite general use in

this country. It was upon this plan that the Steubenville bridge was constructed.

The floor of the main span, on which the train is seen, will be about 100 feet above low water. This span is planned at a length of 420 feet; the one next south is 240 feet, and the others as near 200 feet each as the division of distance will admit. There will be seven spans in all, with the eight piers. Beyond the front streets of both Newport and Cincinnati, the grade to the cities will increase, that of the wagon tracks being much sharper than the longer and easier one of the railroad.

The bridge will be forty-one or two feet in width, with thirteen feet in the middle for trains, one way on either side for cattle and vehicles, and on the outside of these still, the passages for foot-passengers.

This bridge will be built by the Newport and Cincinnati Bridge Company, organized with a capital of $1,200,000, and having the following as its Board of Directors and officers; Alfred Gaither, President; Albert S. Berry, Vice-President; Charles H. Kilgour, Secretary and Treasurer; M. J. King, William Ringo, W. H. Clement and T. G. Gaylord.

They contemplate having a train cross this bridge by the 1st of December, 1869. J. H. Linville, of Philadelphia, is the chief engineer and supervising architect. Mr. John C. Wilson is the resident engineer, with an office at the north-west corner of Pearl and Butler Streets, Cincinnati.

Passage through the Suez Canal.—The Rob Boy, an English merchant vessel, recently passed through the Suez Canal, and the captain writes to the *London Times* the following account of the present condition of this great undertaking, after thirteen years have been spent in its construction:

"The canal, as designed, is about a hundred miles long. Of this length, about half is sufficiently advanced for the sea water to reach fifty miles—that is, into the middle of the isthmus. It is finished to its full breadth, which is a hundred yards, or the width of a considerable river, but not to the intended depth of twenty-six feet. The remaining fifty miles not yet penetrated by the sea water, are in various states of progress; parts are excavated, parts are under water, parts will have to be laid under water which is to be supplied from a great lake not yet filled, while a good many miles have to wait for large blasting operations. To English ears it must sound

promising that a good deal of clay has to be cut through; for nothing can be dealt with so successfully in this country, as that material. The completion of the southern half of the canal would look like a very long work, but for the fact of the immense subsidiary works being completed, and a vast mass of appliances being on the spot. The service canal, from the Nile to the mid point of the salt water canal, and branching thence to either extremity, is an immense work, not less than a hundred and fifty miles long, and in full use for the supply of fresh water for navigation, and for otherwise assisting the work to be done. The port at the Mediterranean end is an immense work, already available. The sea channel at the Suez end has difficulties, but only such as engineers are familiar with. Forty enormous and costly dredging machines are at work on different parts of the canal—chiefly, we conclude, the northern half—discharging mountains of mud, sand and clay over the banks or into barges. The rate of expenditure is put at £200,000 per month, or two and a half millions a year. Our informant calculates that a driving wind, after blowing a month together, will send into the canal, when finished, five hundred tons of sand a day, or fifteen thousand tons a month. This, however, is no more than a single dredging machine would be able to keep down at a certain moderate cost in coal. The difficulty of keeping up the banks of the canal, exposed as they will be to the wash of steamers, and to a surface often agitated by the wind, is a more serious matter, but one which does not enter into the present question. Upon the whole, it does seem a moral certainty that, at least in two or three years—for one year seems out of the question—this great undertaking, worthy of a heroic age, will be brought to what we may fairly call an actual completion. In the course of the year 1871, we may probably see the sea water of one ocean flowing into the other."

Hoosac Tunnel.—It is announced that the Hoosac Tunnel contract has been disposed of, so far as abstract propositions are concerned, and that the executive council are now at work upon the details. Messrs. Shanly Brothers, of Canada, are the successful parties, and their bid is stated to be $4,750,000. The terms of security have been so far modified as to allow the parties contracting to finish work on the tunnel to the amount of $500,000 before drawing from the Treasury in lieu of furnishing the like amount in bonds.

The Denver Pacific Railroad it is stated has been graded the

entire distance, 105 miles, from Denver to the connection with the Union Pacific. The entire work is paid for. The track it is asserted, will be laid early in the spring.

The Tunnel under Chicago River, at Washington Street, was formally opened to the public, January 1st. The event was celebrated by the passing through it of a procession composed of the Mayor, Common Council, Fire Companies and distinguished guests. The authorities consider it such a success that they have advertised for proposals for two more at different points of the river.

Chromium Steel.—It has long been known that an alloy composed of sixty parts of chromium and forty parts of iron is so hard as to scratch glass like a diamond, and such an alloy may be formed by heating oxide of chromium in a blast-furnace, with metallic iron. Experiments are now being made to produce a species of steel, suitable for rails and other purposes, by adding chrome ore and manganese to the iron in the puddling-furnace; and the results are said to be promising, though not conclusive.

Photography and Gunnery.—During the late experiments at Fortress Monroe, photographs were taken of the target from an adjacent bomb-proof, so as to record the exact amount of damage effected by each shot.

Photographing a Tunnel.—We have lately seen a photograph taken by sunlight of the interior of the tunnel which penetrates the summit of the Nevada for the distance of 1659 feet, at an elevation of 7,042 feet above the sea level, the greatest height to which a locomotive has yet attained. The success of this picture is due to the position of this tunnel, through which like that near Bore, on the Great Western Railway, the sun shines for a few days each year. Taking advantage of this circumstance, and also using mirrors by which light was thrown successively upon various points, this picture was produced with an exposure of fifteen minutes, and shows the distant heading with perfect distinctness as well as the long intermediate cavern.

Magnesia Blocks for the Lime Light.—Through the kindness of Mr. James Swain, one of our own citizens, now and for a long time resident in Paris, and from his scientific taste and acquirement thoroughly *en rapport* with all the new things, we have received a number of those blocks of compressed magnesia, about which so much has been said of late in the foreign journals. They are square prisms of about three-quarters inch base, and five-

eighth inch in height, of remarkably even texture, and notable density. Notwithstanding all that has been said in their praise, they do not prove on trial to be by any means equal in powers of resistance (when submitted to the oxyhydrogen flame) to the average quality of lime. With a pressure of about three inches of water at the jet, they are rapidly eaten away, and moreover split by action of the heat. It is just possible that these specimens have suffered some deterioration in their transit across the ocean, though considering their method of packing, and the properties of the material, this is hardly probable. Their one undoubted advantage seems to be their security from injury on exposure to the air. In this respect, having a marked advantage over lime. Except, however, where exceedingly low pressures are employed, their rapid destruction before the jet, more than compensates for the other advantage.

Cement for Stone.—It is stated that an admirable cement is obtained by mixing infusorial silica, such as constitutes the diatoms, for instance tripoli, with the following substances. The infusorial silica is mixed in about equal proportions with oxide of lead; about half a part of freshly slaked lime is then added, and the whole is made into a paste, with boiled linseed oil. This is especially recommended for securing iron-work in marble and other stone.

Pacific Railway.—According to the New York advices, regarding the progress of the Pacific Railway, the work will be completed by next July, and at that time there will be a continuous line of rail from New York to San Francisco, about 3,300 miles, the longest in the world. A European passenger will then, it is calculated, be able to reach Shanghai, China, *via* New York, in thirty-eight days, while the shortest time in which the trip can now be made, going eastward, is sixty days. At a recent meeting at Chicago, an examination would show that goods could be gotten from Asia to Chicago at first hands, at a much less cost than they can be bought in New York, which is 955 miles by rail, and 1,500 miles or so by lake, canal, and the river Hudson to the eastward.

The Heaton Steel.—Most of the English journals have for some time past contained lengthy arguments and discussions on Mr. Heaton's process for converting iron into steel, by running the melted iron into a vessel at the bottom of which has been previously placed, under a perforated iron plate, a quantity of nitrate of soda, the oxygen of which, liberated by the heat, and rising through

the molten mass, burns out its impurities. After a review of the subject, it appears to us that the drawbacks to the process are, the uncertainty of the product and its want of purity.

Suez Canal.—Between Shalouf and Suez, a distance of only 20 miles, about 20,000 men are at work. The workmen are organized in the manner described in this *Journal*, Vol. LV., 380 p.and the work is being pushed with great vigor, steam dredges, asses, mules, men, and camels, all contributing towards their completion. The contractor, M. Levalley, is under a penalty of £8,000 a month, to complete the works by the 1st of October next.

An Explosive Dye.—The artificial saffron, invented by Mr. Millonsweg, of Poblitz, has of late been found to be as easily exploded as gunpowder, though possessing forty per cent. less of projectile force.

Moss Rubber Inking Roller.—The editor of the *Mechanics' Magazine*, London, speaks very highly from his own experience of the above mentioned article, which is invented by Mr. Stephen Moulton, of Bradford-on-Avon, and prepared in the following manner. Vulcanized india rubber is reduced to a powder, then placed in a mould, and subjected to a second vulcanizing heat, which converts it into a mossy substance. This core is now covered with skin of rubber, which is then vulcanized, and the roller is then ready for use.

Detonating Gun Cotton.—Some late experiments by Professor Abel and Mr. Brown, seem to demonstrate that compressed gun-cotton if exploded by a detonating fuse, possesses all the power and consequent advantage of nitro-glycerine, so as to effect blasting without tamping, and to shatter heavy iron plates when fired loosely upon them.

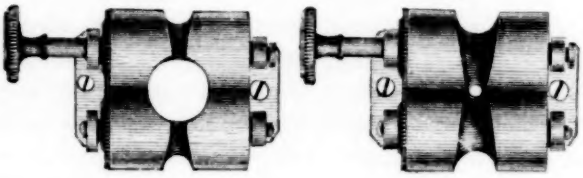
Gradually Adjustable Stop, or diaphragm for lenses. By J. Zentmayer.

At the last meeting of the Institute, there was exhibited this exceedingly ingenious arrangement, which is shown in the accompanying cuts, which are taken from photographs, Fig. 1 showing the apparatus with its largest, and Fig. 2 with its smallest, opening. To obtain a circular diaphragm which, like the eye, should expand and contract gradually by a continuous change. and yet be made of rigid and unchangeable material, might seem, at first sight, to be an impossibility; but, after all, when the result is accomplished, as

in this apparatus, we are surprised no less by the simplicity than by the ingenuity of the means employed.

The wood-cuts almost explain the apparatus of themselves; but we may say in addition, that it consists of two cylinders or rollers with parallel axes and surfaces in contact, having similar conical grooves on their surfaces, and fine teeth cut at one end of each, which, gearing together, cause them to rotate in unison.

Fig. 1. Fig. 2.



There is, theoretically, an objection to a diaphragm of this construction, from the fact that its opening will not always be in the same plane—that is, the smallest cross-section of the space between the rollers will not always be equidistant from a plane at right angles to the line of sight and passing through the axes of the rollers. With the larger opening, this smallest section will be nearest to, and with the smaller, further from, such a plane.

In practice, however, this difference is so small as to be entirely unimportant, and may even, in some cases, be turned theoretically to advantage.

Secondary Batteries.—In a late number of the *Annales de Chemie* we find an elaborate article, of some twenty pages, by M. Gaston Planté, on the above subject. By omitting the historical discussion, and such minute details of experiments as could only be of use to one who wished to make a literal copy of each step, we think that the valuable results embodied in this essay may be expressed in a small space.

If two leaden plates, plunged in diluted sulphuric acid, are made terminals of a small galvanic battery, the one connected with the negative pole, will have its surface rendered perfectly clean and metallic by the reducing action of the hydrogen there liberated, while the opposite plate, connected with the positive pole, will, on the other hand, be rapidly per-oxidized by the nascent oxygen set free at that point. If, then, the battery being disconnected, these two plates are put into communication, they will be in condition to

develop a galvanic current opposite in its direction to that which was before passed through them by the battery. Thus the clean plate or former negative pole will tend to oxidize, decomposing the water of the pile and liberating hydrogen, while the peroxidized plate will tend to absorb this hydrogen and suffer reduction. These actions are precisely such as are needed to produce a galvanic current, which, experience proves to be of a very intense character though short duration. Availing ourselves of this fact, it becomes possible to construct what, from its practical result, may be called a *condenser* for *dynamic electricity;* thus we may employ a small battery, during a notable time, to bring a number of connected lead plates into the condition above described, and then use for a few moments, the intense current they are able to generate, thus obtaining a concentrated and intense momentary effect from a feeble but continued supply of force, not as in the Leyden jar or other condenser of statical electricity, by an actual accumulation and storing up of the feeble force, but by the indirect method and through the intervention of the chemical reactions above described.

M. Planté found it convenient, in his experiments, to employ troughs of gutta percha, each containing 20 plates of lead 10 inches square and $\frac{1}{50}$th of an inch thick, connected as a single pair of elements—that is, the odd numbers of plates united together by a copper strip at one side of the trough, and the even numbers similarly joined on the other side, the trough being filled with a mixture of ten parts of water to one of sulphuric acid. Two such batteries, connected as a single pair, could be fully charged in a few minutes by three small Bunsen cells with carbons 3 inches high (connected for tension), and would then suffice to bring to incandescence thick pieces of platinum wire to fuse short wires of iron, steel, and other metals, to dephlagrate steel needles, and this for a sufficiently long time to burn up a steel needle four to five inches long. By careful measurement, it was, indeed, proved that the electro-motive force of each couple in this secondary battery—during its time of maximum action—was equal to ·2362, that of the Bunsen element mentioned above, being 0·164; in other words, the electro-motive force of these elements was, to the Bunsen, as 1·44 to 1. With regard to resistances, these were nearly equal, being as 27 to 23. By placing each pair of plates in a separate trough, and employing an extensive but simple commutator by which they

could be connected in quantity to receive the current of the exciting battery, and in series to obtain their own current, effects of very great intensity were secured; thus a platinum wire 6 feet long and 100th of an inch in diameter was heated to incandescence; a similar steel wire was instantly fused, the combustion of mercury and the electric light was readily produced, and, in a word, all the effects which could be obtained continuously from a pile from 55 to 60 Bunsen elements of equal surface were secured, intermittently, from this secondary pile. By another arrangement of the commutator a constant series of successive currents was obtained.

This had been accomplished before by M. Thomsen, of Copenhagen, who, in 1865, arranged an apparatus on this general principle for operating a telegraph line.

While the subject of secondary batteries was receiving such development abroad as is indicated by the above abstract, the same branch of electrical research was pursued in this country by Dr. George Percival, and has been brought by him to a point of decidedly higher efficiency as regards practical results and theoretical perfection.

Considering that where both electrodes of the secondary pile were of lead, a loss of effect was experienced, by reason of the low electro-motive force of this metal, he proposed to make the positive electrode of the secondary series, of some more active metal, such as zinc, and then to use as electrolyte, not sulphuric acid and water, but a neutral solution of the positive metal. Thus: with zinc as positive element, a solution of sulphate of zinc; with copper, sulphate of copper; or with iron, sulphate of iron. By this means, by the action of the charging battery, not only is peroxide of lead deposited on the negative electrode of the secondary (which is, of course, connected with the positive pole of the charging battery, and is the positive electrode with reference to the charging current), but there is also a deposit of finely divided zinc on the positive element, and a liberation of sulphuric acid in the solution. By this means, a much more energetic chemical and electrical action is developed when the charging current ceases, than if both electrodes were of lead, and the positive element therefore of this feebly active material.

It is found best to amalgamate the zinc plates of this battery, and as this renders them brittle, it is necessary that this should be done after all their connections are made, and, in fact, the entire

arrangement ready for use. To accomplish this, the series of plates is connected wtth another battery, so that the zinc plates form the negative electrode, and plunged in a solution of 3 ounces of bisulphate of mercury and 8 ounces of sulphate of ammonia in a quart or so of water.

When one of these secondary batteries is charged, it may be removed from the solution and dried, and will then retain its capacity for developing a powerful current for many months.

Fire-Alarm, by M. Diou. At the last meeting of the Institute there was presented by Mr. J. Demorat, for exhibition, an improved fire detector, manufactured by the American Fire Detector Company, 725 Broadway, New York.

This apparatus consists of two parts, one of which is placed in the location where the presence of fire is to be detected, and the other where the alarm is to be given. These are connected by wire, in the manner of ordinary bells, except that the wire is tightly stretched in its normal condition.

The first instrument consists, essentially, of a catch, (holding one end of the wire) controlled by a copper helix, whose expansion will liberate the catch, and thus slack the wire. The other instrument consists of an alarm bell, operated by clockwork, which goes into action as soon as the wire is slacked.

By changing the tension of the copper spring, the instrument may be set to go off at any temperature, indicated by a dial and pointer attached to the regulating screw.

When exhibited to the meeting, the instrument was started by holding it, for a moment, over a gas flame, and by the mere warmth of the breath.

Lining for Fire-proofs.—There was exhibited, at the last meeting of the Institute, a new material, which, by its remarkable power of non-conduction, presents especial advantages. This was devised and patented by Mr. W. Alford. It consists of a rough *papier maché*, made of old wall paper, by moistening and compressing it. Its power of resistance to fire was illustrated by a specimen exhibited on this occasion, which had been exposed, as a lining to an iron box, with a wooden one in the centre, to the heat of a brightly burning anthracite fire, for the space of an hour. The material was charred on the outer surface, to the depth of about one-fourth of an inch, while all the rest, and the box, of course, within, was perfectly intact. This substance has been favorably

reported upon by many of our safe-builders, and seems to be an admirable invention. A melancholy interest is attached to the subject, from the fact that the day on which these specimens were left at the Institute by Mr. Alford is the last on which he has been seen by his family or friends. No sort of clue has been found to this most mysterious disappearance, which would seem to indicate some fatal accident.

A new Spectrum Discovery.—Nearly two years ago, Mr. Norman Lockyer—the editor and translator of "Guillemin's La Ciel," ("The Heavens," in its English form,) author of an excellent little book entitled Elementary Lessons in Astronomy, which contains some of the best engravings of the sun and planets which have been published, and a young astronomer of high reputation—announced a plan by which he proposed to examine the luminous rose colored prominences seen around the sun during total eclipse, by aid of the spectroscope, on ordinary occasions, and without the necessity of an opaque body to cut off the sun's general light.

It was founded upon the following considerations: If these prominences were composed of intensely heated gas, they would produce bright lines in the spectroscope. Now, these lines, being each purely monochromatic, of a fixed refrangibility, or non-composite nature, would not be dispersed or spread out by any number of refractions, hence their intensity of light would remain but little impaired after a great many refractions, or passage through a large number of prisms. The general white light of the sun, however, would, on the contrary, be continually dispersed in proportion to the extent of its refraction, and might thus be finally diffused and outspread by this means, until its brightness no longer masked that of the relatively faint and imperceptible lines due to a heated gas.

With this view, he constructed an instrument, and made observations of the sun, hoping to see a gaseous spectrum superposed upon the solar one, or continuing it, along the edge of the sun's disk. In this, however, he failed, and was thus led to suppose that the red clouds or flames, might not be gaseous after all.

The observations made during the late eclipse, however, proved that these prominences *were* gaseous, and developed bright lines. He therefore justly concluded that his ill success was chargeable to his instrument, and by improvements in this, he was enabled to demonstrate at once the truth of his theory, and some most im-

portant facts in regard to the constitution of the sun. This successful observation was made on the 20th of October, with an instrument constructed by Mr. Browning. The slit of the spectroscope being turned to the edge of the sun's disk, a solar spectrum, due to the light from the disk, is seen, and upon this another, of bright lines, due to a luminous gas. If a luminous prominence of some height is situated at this point, the gaseous spectrum is of considerable height, and by carrying the spectroscope around the edge of the disk, and, noting the height of the gaseous spectrum at different points, the shape of the luminous prominences may be mapped out.

Mr. Lockyer, however, finds that a gaseous spectrum, of a certain height, is present at all points, and hence concludes that an envelope of luminous gas surrounds the sun to a distance of about five thousand miles, while the luminous prominences are only accumulations of this same gaseous matter, reaching sometimes a height of seventy thousand miles.

On the 18th of August, Mr. Jansen, of the French observing party, sent to India to make record of the total eclipse; made a similar discovery as to the possibility of observing the prominences without an eclipse. While, therefore, Mr. Lockyer was the first to propose the method, Mr. Jansen was the first to make the actual discovery, though Mr. Lockyer did the same thing, independently, afterwards, before any account from Mr. Jansen had reached Europe. The honor of the discovery might then be fairly divided between these two physicists.

Spectrum observations during the late *eclipse*, of which a full account, with a colored plate, appears in the last number of the London *Quarterly Journal of Science*, seem to indicate that the prominences or flames consist of intensely heated hydrogen, and of some body giving a yellow line, very near to that of sodium. The lines on which the various observers seem to agree best are a red one at or very near a yellow at or near D, a blue at F, and a violet at G.

Civil and Mechanical Engineering.

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 43.)

Deductions and conclusions drawn from Table.

"Pulleys covered with leather, with grain-side of band to pulley, will sustain 50 *per cent. more resistance* than without the pulley being covered. The per cent. of resistance of the bands on the different pulleys is nearly as follows; and this per cent. will indicate the relative working value of each pulley respectively:

Iron pulley covered with leather	36 per cent.
" polished	24 "
" rough turned	15 "
Wood pulley, polished mahogany	25 "
	100 "

"Full 6 per cent. should be added to the polished iron pulley, to make allowance for the difference between commencing to slip and its sliding; thus making polished pulley 30 per cent., or next in value to leather.

"The relative or comparative working per cent. of the different bands, as indicated by the Table, is nearly as follows:

Leather, grain-side to pulley	31 per cent.
" flesh " "	23 "
Rubber	21 "
Gutta Percha	14 "
Canvas	11 "
	100

"Thus leather belts, grain-side to pulley, will drive 34 per cent. more than flesh-side to pulley; 48 per cent. more than rubber; 121 per cent. more than gutta percha; 180 per cent. more than canvas; consequently, the very best arrangement for belting is to use it with grain-side to pulley, and have the pulley covered with leather. This is best in all cases. The next best pulley is polished iron, especially for quick motions. Polished wood next, and rough iron least in value.

"Leather, used with grain-side to pulley, will not only do more work, but last longer than if used with flesh to same. The fibre of the grain-side is more compact and fixed than that of the flesh, and more of its surface is constantly brought in contact or impinges on the particles of the pulley. The two surfaces, that of the band and that of the pulley, should be made as smooth as possible: the more so the greater the contact surface, and the more the particles of each impinge on the other. The smoother the two surfaces, the less air will pass under the band, and between it and the pulley—the air preventing the contact of band with pulley—the greater this contact, the more machinery will the band drive. The more uneven the surface of band and pulley, the more strain will be necessary to prevent bands from slipping. What is lost by want of contact, must be made up by extra strain on the band, in order to make it drive the machinery required—oftentimes, if the band is laced, causing the lacings to break, the holes to tear out, or fastenings of whatever kinds to give way.

This want of contact is noticeable on most of new bands used with flesh-side to pulley, and is distinctly marked by dark impressions on the band where it comes in contact with the pulley. Oftentimes not half of the surface will be found to have come in contact, and until it is worn smooth, or filled in with other substances, the full extent of the power of the band is not obtained.

* * * * * * * * *

"Bands used with grain-side to the pulley will never crack; as the strain, in passing it, is thrown on the flesh-side, which is not liable to crack or break, the grain not being strained any more than other portions of the band.

Rule for determining the width of belts.

$$w = \frac{5334 \text{ H P}}{\text{V C}}$$

In which w = width of belt in inches.
H P = horse-power transmitted.
V = velocity of belt per minute in feet.
C = part of circumference of smaller pulley in contact with belt, in feet.

Patent Angular Belting.

"This invention consists of a novel belt of a trapezoidal form, to be used in connection with a V-shaped or angular grooved pulley,

for driving all kinds of machinery where belting is required to transmit power.

"The angular belt is a great improvement on the round, square, or flat belt; a much greater surface of the belt is brought in contact with the pulley than with any other kind. It will wedge itself into the groove and resist any slipping action during the rotation of the pulley, so that the greater the strain put on one side of the belt the tighter will it be held in the groove; not being liable to slip on the pulley, it may be used very loose, causing less friction, consequently requiring less power to drive machinery, and giving it greater certainty and regularity of motion. As the angular band is not liable to slip, it can be used with greater economy and certainty than either the round or flat belting; its power is limited only by its strength; made of the same materials and the same width of belt, this form has more strength than any other.

"This belt has been used with perfect success, when other bands have failed entirely to impart motion to machinery. They are made without a joint in their length; and when the width requires more than one thickness of leather, the belt is connected, then rivetted or screwed, so that the fastening will not come in contact with the pulley."

Reasons why Rubber, Gutta Percha, and Canvas are the dearest articles to use for Bands.

"Under the same circumstances, and on the same machines, these bands will not last or wear one-fourth as long as leather. When once they begin to give out, it is next to impossible to repair them.

"Wide bands cannot be used for or cut up into narrow ones, as leather can be.

"Leather belts may be used over and over again, and, when of no further value for belts, can be sold for other purposes.

"A rubber band, costing hundreds of dollars, may be spoiled in a few moments, by the lacing giving out, and the band being run off into the gearing, or by being caught in any manner so as to damage the edge, or by stoppage of either the driving or driven pulley. A few moments of quick motion or friction will roll off the gum from the canvas in such quantities as to spoil the band.

"Leather belts may be torn or damaged, yet are easily repaired.

"Should a rubber or gum belt begin to tear by being caught in

the machinery, if the rent strikes the seam, it is most certain to follow it, even the entire length, if the machinery is not stopped. It would be impossible to tear leather in like manner.

"Oil, in contact with rubber belting, will soften the gum. * * *

"Rubber, gutta percha, and canvas belts will contine to stretch as long as in use, rendering it necessary to shorten them continually.

"During freezing weather, if moisture or water finds its way into the seams, or between the different layers of canvas composing these bands, and becomes frozen, the layers are torn apart, and the band is spoiled; or if a pulley becomes frosty, the parts of band in contact with it will be torn off from the canvas and left on the pulley.

"Gum belts will not answer for 'cross' or 'half-cross' belts, for 'shifting' belts, 'cone pulleys,' or for any place where belts are liable to slip, as friction destroys them. * * * * * * * * *

"A well-made leather band, if properly looked after—the width and pulley surface proportional to the amount of work to be done—will last 12, 15 or 20 years, and yet be of value to work over into narrow belts."

(To be continued.)

WOODEN RAILWAYS

By D. S. Howard, C. E.

Notwithstanding the retrograde appearance in the use of wooden railways, they are fast forcing themselves into favorable notice. Since the first one built in this country, in 1860, by Mr. J. B. Hurlbert, for the exclusive use of his lumber mill, situated at Hurlbertsville, about five miles north of Boonville, on the Black River Railroad and Canal, public opinion seems to have been very favorably influenced toward them. Mr. Hurlbert has since constructed a similar railway, twenty-four miles long, from the Clifton iron ore beds in St. Lawrence County, New York, to the Ogdensburgh Railroad, which is now in successful operation, and has been examined by a correspondent of the Philadelphia *Press*, which publishes, from his notes, a very favorable account of its usefulness. An agent has also been sent from Canada to examine into its utility, whose report is also very favorable, as republished in the *Portage Lake Mining Gazette*, by which we are informed that a

company has already been formed, and directors chosen, for the purpose of building a similar road from Houghton and Hancock, on Portage Lake, in a northerly direction, twenty-five miles along the Mineral Range, for the use of the mines now open on the route.

There is also a company formed for the purpose of constructing a similar road from Carthage, Jefferson County, New York, northeasterly *via* Harrisville to St. Lawrence County, forty-seven and a half miles, a route well calculated to develop the rich iron ore district about Bonaparte's Lake, near the situation of the Alpine Iron Works, to the Ogdensburgh Railroad. The Black River Railroad, now completed to Lowville, will soon be extended to Carthage. This will be a great inducement for the two companies to iron the extended Harrisville Road, thus completing the nearest route from New York City to the Canada line.

One of the favorable features of the wooden railway is, that not a blow struck in the construction is lost in converting it into an iron road, it being one of the perfect progressive improvements.

Cost of Construction.

The Clifton Road cost $7,000 per mile, through a wild, mountainous region, traversed by deep ravines, rocky ridges, and tortuous streams, one of which the road crosses eleven times in a mile. The grades are necessarily very high, having to rise 1,092 feet in 22 miles. In some places they are 300 feet to the mile. The curves, also, are of very short radius, some of which are not more than 450 feet.

The Carthage and Harrisville Road will probably be built for less than $5,000 per mile. The route is somewhat uneven, but not mountainous, and the two termini nearly on the same level with a surrounding country mostly settled.

The Portage Lake and Keeveenaw Railway route is probably in a more feasible shape still, and, were it not for other considerations, the road could be built much cheaper than the Carthage and Harrisville Road; but as no such work can be done there in the winter season, labor and provisions must be imported every spring for such purposes.

The following figures will show the estimated cost for one mile of the superstructure of the Carthage and Harrisville Road, which will vary in the construction of other roads, as the prices of labor and materials differ in other localities. The grading must be esti-

mated, on all routes, according to the grade required, the character of the material, the contour of the country, &c.

1,760 ties, 3 feet from centre to centre, at 10 c.	$176 00
21,120 feet B. M. 4×6 maple rails, at $15 per M.	316 80
3,520 wedges, at 10 c. each	35 20
192 days' work, 5 rods per day for 3 men	382 00
10 per cent. for superintendence	91 00
Total cost of superstructure	$1,001 00

The grading costs from $2,000 to $6,000 per mile. The whole cost of the first road of the kind built by Mr. Hurlbert, mentioned above, was less than $3,000 per mile, which is less than a good common road could have been made on the same route, with the same grade. The capacity of the two roads, at the same cost of construction, repairs and operation, is as one to twenty-five.

The wooden railway is available at all seasons of the year, and may be made capable of carrying any load required, by increasing the width of the wheel and rail.

Mode of Construction.

Round ties, about one foot in diameter, are bedded into the grade, three or four feet apart, according to the size of rail to be used, or load required. These ties are notched to a proper gauge to receive the rails, which are fastened by driving a hard wood key on the outside of the rail, so that when any part is too much worn or defective in any way, it may be readily replaced. The butt-end of the largest ties are so placed alternately as to receive the ends of the rails where they meet on opposite sides. The meeting of the rails on both sides should never come on the same tie, which, in such a case, has a tendency to rock back and forth in its bed as the load approaches and recedes.

These roads are so eminently useful in so many instances where a limited private business is carried on, such as lumber mills of all kinds, iron works, paper mills, and all manufactories requiring the transportation of large amounts of raw material and heavy products, that we may soon see a marked difference in the success of these various operations, as well as the commencement of many others that could not be carried on at all without such facilities, thereby increasing the wealth of the country in proportion to the dormant energies they thus bring into requisition.

Lyons Falls, December 16, 1868.

NITRO-GLYCERINE.

PUBLIC attention has of late been directed to the value, and to the danger in its use, of *Nitro-Glycerine* as an agent to replace gunpowder or gun-cotton in blasting, and several papers have been recently published, that have very elaborately treated the subject.

It is therefore intended to confine the present communication to a simple description of the peculiar method of manufacturing this oil, recently invented by Stephen Chester and Otto Bürstenbinder, and to the statement of some of the facts, that have been developed in a series of experiments made by them, which culminated in their adopting the method of manufacture to be described.

Preliminary to this, a few words may well be quoted from an interesting article by Mr. E. P. North, which was read before the American Society of Civil Engineers, in March, 1868.

"Nitro-Glycerine was discovered in 1846 by Sobrero, but nothing was done with it until 1863, when Alfred Nobel patented its application to blasting. * * * Nitro-Glycerine is made by treating glycerine, which has the formula $C_6 H_5 N_3 O_{18}$ with nitric and sulphuric acids, as in the case of gun-cotton, and the chemical reactions are nearly the same, it being in both a case of the substitution of nitrous acid for a part of the hydrogen. By explosion, according to an article in the *London Mechanics' Magazine*, September, 1865, one volume of oil is converted into

Carbonic acid gas	429	volumes.
Steam	554	"
Oxygen	39	"
Nitrogen	236	"

In all, 1,258 volumes for one volume of liquid oil, being thus theoretically five times more effective than the same bulk of gunpowder."

But it is believed that the effective force of nitro-glycerine in rock blasting is really very much greater than this, as the writer will hereinafter endeavor to show.

The formula for combining the several ingredients from which "blasting" oil is produced, will of course vary with the quality and intensity of acids and oil used.

With such acids and combinations of them as the inventors have

been able to procure hitherto, they have found the best results from the following combination:

By weight—sulphuric acid	.....	6 parts.
" nitric "	.....	3 "
" glycerine oil	.....	1 "

From this the average product will be 1·5 parts (by weight) of nitro-glycerine, having a specific gravity of 1·6.

The amount of oil produced is in great measure dependent upon the relative freedom of the acids and oil used from water, and not upon the chemical purity of the ingredients; and it also greatly depends upon the uniformity of temperature preserved during the process of mixing the oil with the acid. Should the temperature be too high, oxalic acid will be formed, and the heat produced by the sudden commingling of the oil and acid, will at any time cause deflagration. On the other hand, if the oil be but partially congealed (and it freezes at a high temperature), the solution of the oil by the acid will not be complete, and in that case will be dissolved and washed away during the precipitating process.

The inventors have produced from the formula above given, 1·8 parts, while under other circumstances the result derived from the same mixture has been less than ·2 part.

But the affinity of the strongest sulphuric acid for water is so great that the absorption of water from the atmosphere during the mixing, materially affects the product if it be exposed to this contingency.

The peculiarity of this invention exists especially in the method of preserving uniformity of temperature, excluding the atmosphere from the acids, and intimately commingling the oil and acids, and causing the complete solution of the former.

Upon a small revolving table, is placed a jar of capacity to contain, say, twelve or fifteen pounds of the sulphuric and nitric acids previously mixed. In this is suspended a glass tube, with radial branches of diverse lengths, in several horizontal planes, but terminating in fine tubular points, turned to the right or to the left, as the case may be, in such manner that the axes of the capillary orifices in the terminations of the several arms or branches, shall be tangential to the arcs described by the respective arms, from the vertical tube as a centre.

By a tube or pipe, connecting the vertical tube with a reservoir of compressed *carbonic acid gas*, gas is at pleasure forced through

the several orifices described, causing the body of liquid to rotate within the jar, and against the radial arms, while the escaping gas, by reason of its relative gravity, rests upon the surface of the liquid, effectually excluding the moist atmosphere.

But the sudden expansion of the compressed gas within the liquid takes from its caloric, and as the escape is through many small orifices, and not directly upward, the particles of liquid are rapidly and successively brought in contact with the expanding gas, and its cooling effects are immediate and controllable, since a stop-cock at the hand of the operator regulates the flow of gas at will.

On the other hand, great heat is produced by the contact of the oil with the acids. Hence, a similar stop-cock, regulating the flow of the oil into the acids from its reservoir, enables the operator to *immediately* raise or lower the temperature, as the raising or falling of a thermometer suspended in the liquid, may indicate, that one or the other stop-cocks should be turned forward or back.

Practically, it is found that the thermometer may be kept vibrating between two degrees, with but little trouble, while the oil is rapidly introduced. Also, that by this means, five pounds of oil may in one vessel be mixed with the corresponding amount of acids in less than forty minutes. It also seems that by this method, that the solution of the oil is immediate, and much more complete than when more slowly mingled in ordinary atmosphere.

This may, however, be due in part to causes other than those alluded to, not yet investigated, but due to the presence and use of carbonic acid gas in the manner described.

The ease with which the temperature is controlled, is such that the operator may readily attend to several vessels, all supplied from the same gasometer, but from separate fountains of oil at the same moment; and as it is found that it is convenient to mix in vessels of sixty pounds capacity, it follows, that under favorable circumstances, one operator can mix what will produce at least twenty pounds of nitro-glycerine every forty-minutes.

This apparatus may all be contained in a box easy of transportation, and which, when the apparatus is taken out, answers for table and ice-box, for cooling the gas cylinder, when the weather is very warm.

After the union of acids and oil is complete, the mixture does not present an oleaginous appearance, and the completeness of the

chemical union can only be guessed at by the clearness of the mixture. This should then be mixed with twenty times its bulk of water, in which it apparently unites, forming nearly a clear fluid, without oily appearance. In twelve hours, however, nitro-glycerine will precipitate, and as its specific gravity is 1·6, will be found on the bottom of the vessels, and the residue of acids and *oil* not previously *consumed* (if this term may here be used) by the acids, may be poured away with the supernatant water.

Hence, large quantities of water are required.

It has been found that after *eight* or *ten* days, decomposition will take place in this product, slowly it is true, but with a rapidity proportionate to the amounts of free acid left. Hence, when it is required to pack and store this material, it is absolutely necessary that after precipitation, as before described, that it shall be many times washed in large bulks of water until no trace of acid remains.

Another curious and important fact has been noted in the experience of the inventors of this method of manufacture. Previous to any decomposition having taken place, it is with extreme difficulty that this oil can be caused to explode, though its expansion *when* exploded seems to be many times greater than that which has been kept for some time. The following experiments will illustrate.

1st. 1½ ounces were placed in a vessel at the bottom of a hole, and over it was placed 2 ounces of rifle powder, in which was inserted a common time fuse, and the whole buried two and a half feet in sand. The powder exploded, forming quite a crater, but the oil was not exploded.

This was repeated several times, with the same result.

2d. Acid and glycerine oil were suddenly dashed into a vessel containing nitro-glycerine. Great heat was developed, and the mixture took fire and burned. There was no explosion Owing to the want of facilities, as these experiments were tried in a sand barren, it was not ascertained whether the nitro-glycerine was consumed, but it is believed that the mixture only burned until the uncombined oil was consumed.

3d. Percussion caps, exploded within the nitro-glycerine, failed to explode it; and it could only be made to explode by confining it, and using extra large cartridges of detonating powder exploded within it.

4th. Equal quantities of this oil, and of some which had been

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packed in tin cans (apparently having been thoroughly washed, as no visible decomposition had taken place), were placed in similar positions, but though the old oil could be exploded with great ease, in fact, being very dangerous to handle at all, yet in every case, the effects produced by the new oil seemed to be many times greater than that produced by the old oil.

At Hamburg, in May, 1865, *new* oil, prepared by Mr. Nobel, was poured into an iron cartridge, and the attempt made to explode it by use of common blasting fuse. In all cases, it resulted that so long as the combustible portions of the fuse remained in the oil, that it burned quietly, and ceased burning as soon as the fuse was removed.

A new pistol-barrel eighteen inches long was partially filled with the same oil, and after inserting the percussion fuse, the filling completed with loose sand, and thus prepared, placed in a stout wrought iron gas pipe, three feet long, and two inches in diameter, one end of which was firmly planted in a clay soil. Upon explosion, the earth was lifted several feet high, the gas pipe was torn in three parts, and in the ground a cavity four feet deep, and three feet wide was found, but no piece of the barrel was recovered.

At Wamsbeck, about the same time, a small drill hole in an iron anvil weighing over 300 pounds, was charged with one-tenth of a pound of oil. The anvil was shattered into fragments.

Near Stockholm, in 1865, the following experiments were made with Nobel's Nitro-Glycerine, *newly made*, especially for the purpose of testing the danger of its use as compared with any other explosive compound.

1st. A red hot iron bar was repeatedly drawn through nitro-glycerine poured upon a smooth, flat stone, at first without effect, but finally, when the oil and the surface of the stone became heated, the oil took fire, and part of it burned with a flame, without explosion.

2d. A burning shaving was immersed and moved about in the oil. Though the oil could be made to burn, it ceased to do so when the shaving was withdrawn.

3d. Three bottles filled with the oil, were first heated to 120° F., and were then dashed to atoms upon the rocks, but no explosion took place.

The mass of testimony of American engineers—too voluminous to quote at length—seems corroborative of the fact, that the newly made oil can with difficulty be exploded by violent concussion, accompanied by great heat, and that the extremely sensitive charac-

ter sometimes exhibited, is only developed, after being kept some time, probably influenced by changes of temperature. It also seems that the effective force of the oil decreases in proportion as the sensitiveness to ignition increases.

The object of the inventors, however, has not been the production of an explosive oil of greater strength or better quality than the nitro-glycerine heretofore produced, and though some of the results of experiments tried by them would seem to indicate that it might be so, this is no part of their claim, their object being to render the manufacture of the article so simple and safe that it can at all times be made in all desirable quantities, in the immediate vicinity of the points where it is to be used, thus securing the following direct and unquestionable advantages.

1st. Removing the necessity of packing, storing, and transporting a material which grows more and more dangerous the longer that it is kept.

2d. Supplying a means of manufacture unattended with a shadow of danger, other than in the mere handling of acids.

3d. Dispensing with the labor and time required to thoroughly wash the material, as it may be poured directly from the precipitating jar, into the hole or centre, where it is to be exploded, any time within ten days after the oil has been mixed.

5th. The opportunity afforded to use the oil, when it is unquestionable that its effective force (whether this is due to the process of manufacture, or merely because newly made), is very much greater than when it has been kept or packed, while the danger of handling it is proportionally less, as it then *cannot* be exploded when *unconfined*, and with difficulty (as compared to powder or oil that has been packed), when confined.

HISTORY OF THE INVESTIGATIONS ON THE RESISTANCE OF SOLID BODIES.

By De Volson Wood.

The French have made a very full investigation of this subject, and, in their records, have not been satisfied with merely sketching its outlines, but have entered very fully into its details, and given all the points of interest brought forward by each writer and experimenter. An abridgment of this history, covering 220 pages, is

given in the introduction to *Navier's Résistance des Corps Solides*,* an abstract of which constitutes the following article. The history of the early developments of a science, even in its minutest details, is more interesting than the voluminous productions which trace it in its broader growth. Accordingly, I have given quite a full abstract of the early history, and barely touched upon the later portion.

The architects of the middle ages have not left any traces in their writings of the manner by which they determined the proportions of their structures. All their investigations were upon the equilibrium of external or applied forces, without even considering the resisting forces at the supports, or at the fixed points, and much less the internal resistances of the fibres or elastic resistances.

For instance, they did not consider that *ten-pins* were maintained in a vertical by their proper position, but by the equilibrium of all the external forces.

They were fond of determining the resistance of blocks to crushing; and before using a new kind of stone, they probably made experiments by the aid of levers.

In regard to carpentry, they reasoned upon the combination of timbers which constituted frames; but the low price of timber permitted them to use pieces of large dimensions. But we may presume, however, that they knew that a prismatic piece, loaded transversely, was stronger in proportion to its depth than to its length; for the principal object of Galileo seemed to be, at first, to determine the *theoretical law* of this resistance, as well as the ratio between the *absolute* resistance which is exerted when a piece is broken by a tensile strain, and the *relative* resistance when it is broken by a force perpendicular to its length. After his visit to the arsenal at Venice, he considered the cause of the failure of large machines when small ones of the same kind had succeeded.

At first he considered a horizontal cylinder or paralopipedon, having one end fixed firmly in a wall and a weight acting on the other. He supposed that all the points of the beam in the section of the plane of the wall resisted rupture equally, and hence the horizontal resultant passed through the centre of this section, and was in equilibrium with the weight at the other end of the beam, by means of a right angled lever, of which the point of support was at the lower edge of the section of rupture.

* Resistance of Solid Bodies, with notes and appendix, by M. Barré-de-Saint-Venant. Published by Dunod. Paris, 1864.

The forces being reciprocally proportional to the arms of the lever, he concludes that the *absolute* resistance is to the *relative* as the horizontal arm, or the projecting length of the beam, is to its vertical arm, or half depth of the beam.

From this Galileo easily deduced that a rectangular piece pressed sidewise resisted more than the same piece pressed flatwise, in the ratio of the depth to the breadth of the *section of rupture;* that the weights capable of being supported by pieces which have their ends fixed and the weights supported at the free ends, or whose ends rest on two supports, and the weights supported in the middle, *are as their breadths, as the square of their depths, and inversely as their lengths;* also, *that hollow cylinders, such as bones, quills, the stalks of plants, &c., resist much more to transverse stress than solid cylinders of equal volume*, in a proportion which he determined, which caused him to reflect on the works of creation and considerations of an elevated order.

Finally, flowing from the same principles, he showed that a piece which has one end firmly fixed, and whose inferior surface is a horizontal plane, and whose vertical longitudinal sections are equal parabolas, the vertices of which are at the end where the weight is suspended, is equally strained at every vertical section, from one end to the other. Such a piece is called a *solid of equal resistance*, and gives the most economical distribution of the material.

Galileo's hypotheses were erroneous in supposing that the strains were equal at all the points in the section of rupture, and in placing the support of the lever at the base of the section of rupture; and yet all the theorems which he deduced have been confirmed by later researches, except that which gives the ratio between the resistances to rupture by a longitudinal and by a transverse pressure.

Galileo has been charged with ignorance of the principles which enables us to determine the form of beams of uniform resistance, because he stated that "We may diminish the volume of a rectangular beam more than $\frac{33}{100}$ without diminishing the strength, which is a great consideration in the construction of ships;" but the architect, Francis Blondel, detected the error of this statement, by showing that if the beam is to resist a load applied *at any point*, the longitudinal section should be elliptical; but, in this case, as Navier has remarked, there will be an excess of strength at every section, except the one directly under the load.

The principles of Galileo, as thus understood, were applied by P. Fabri, Vincent, Viviani, and especially by his follower, P. Grandi; but about the same time, Hooke, a noted physicist of England, and Marriotte, a noted physicist of France, announced the basis of the true theory of the rupture of solids.

In 1678, Robert Hooke, in pursuing his studies of steel springs, especially of watches, gave his famous principle *ut tensio sic vis*, which he says he discovered in 1660, but which was first announced in 1676, under the anagram *c e i i n o s s s t t u u*. He based the theory of elasticity on the proportionality of the extensions or contractions to the forces which produced them, and thus explained the vibratory movements of bodies, and added the fruitful remark, *that when elastic bars are bent, the material resists elongation on the convex side, and compression on the cancave side.*

Near the same time, in 1680, Marriotte, a Frenchman, having discovered, by several experiments on wooden and glass rods, that the ratio between the resistance to rupture by a pull and by a transverse strain was greater than that given by Galileo, ascertained *that hard bodies, elongated more or less under the action of different weights, and very nearly proportioned to those weights*, and *when the weights were removed they returned to their original length*, and that they are also compressible, so that a stick which is bent contracts on the concave side, and extends on the convex side; and he added, that *it is reasonable to suppose that this compression resists as much as the extension;* finally, the extended parts rupture only because they are *extended* beyond what they are able to resist.

We here see a correct statement of the essential principles of flexure as now understood. Galileo did not recognize any extension of the fibres; and Marriotte supposed that extension always preceded rupture. But the latter made an error in the application of his principles; for he supposed that the resistance to rupture is the same as if the section turned about the base, and the strains increased uniformly from zero, on the lower side, to that producing rupture on the upper side; and hence, in a rectangular section, the resultant will be at $\frac{2}{3}$ the depth, and the total resistance $\frac{1}{2}$ what it would be if all the fibres were equally strained. Accordingly, the *absolute* strength would be to the *relative* as the length to $\frac{1}{3}$ the depth, instead of $\frac{1}{2}$, as found by Galileo. Afterwards he definitely stated *that the axis of equilibrium, on which the fibres are neither extended nor compressed, is at the middle of the depth;* but he still

erred in assuming that the resistance was the same as if all the fibres were all extended. The axis of equilibrium M. Dupin called the *line of invariable fibres*, and Tredgold, and more recent writers, *the neutral axis.*

James Bernoulli, in his writings of 1705, claimed that he was the first to give the true hypothesis for calculating the resistance of solids. Until recently, writers generally gave him the credit of being the first to recognize the compressive resistance. But his theory was the same as Marriotte's, with one unfortunate change, consisting *in regarding the extensions and compressions as augmenting in a less ratio than the forces which produce them.* He endeavored to establish his position by saying, that if compressions were proportional to weights which cause them, a rod would be compressed more than its whole length; and he added that it would be the same with extensions, as they are only negative compressions; and he tried to confirm it by some slight experiments on certain cords, which he found did not follow the law of proportionality rigidly. His reasoning would have been correct, were it not that the elasticity of all materials is changed for a sensible amount of extension or compression.

Bernoulli also erred the same as Marriotte, in affirming that the transverse force of rupturing was the same, whether the fibres are all extended or all compressed, and that it made no difference where the fixed axis is supposed to be, whether at the top of the section or bottom, or at any point between. He failed to perceive the equality of the extensive and compressive forces, and hence could not fix the position of the neutral axis.

In July, 1684, two months after the death of Marriotte, Libnetz, who had learned of his predecessors' investigations and experiments, admitted, like Marriotte and Hooke, the law of the proportionality of the longitudinal stresses, but placed, like his predecessors, the axis of rotation at the base of the section.

Varignon, in 1702, presented a *Memoir on the Resistance of Solids*, in which he placed the neutral axis at the base of the section, and assumed the extensions an unknown function of strains, and deduced a general formula of resistance; then *assumed* different laws, and compared the results with previous experiments, and thus tried to ascertain the most probable one.

The acamedician, Parent, who has not received the notice which he merits, appears to have been the first to make a correct application

of the principles of Marriotte. In his earliest memoirs he made the same errors as his predecessors; but, in a later work, about 1713, he took the moments about the *neutral axis*, and showed that the *sum of the moments of extension and compression gave a result only half the amount found by Marriotte and Bernoulli. He also proved that the sum of the resistances of the compressed fibres would equal that for the elongated ones*, a property before mentioned, and which enables us to establish the position of the *neutral axis*.

Bilfinger, a few years later, gave to Marriotte the credit of being the first to consider compressions; and Coulomb, in 1773, presented his celebrated memoir on the resistance of solids, in which we find laid down nearly all the bases of the theory of the stability of constructions. He established himself on correct principles by using the principle of Statics—then but little known—that the algebraic sum of all the forces must be zero on the three rectangular axes; and the sum of the moments about a fixed point are zero. With the former principles he determined the position of the *neutral* axis; and, with the latter, the ratio between the *relative* and *absolute resistance*. He entirely rejected the perfect rigidity of the material, and considered it elastic, and stated *that if the law of proportionality of extensions and compressions did not hold good up to the point of rupture, the neutral axis would change positions*. No real advance was made in the *theory* for forty years after this essay.

Barlow, in his "Essay on the Strength of Timber," as late as 1817, to determine the position of the *neutral axis*, assumed *the erroneous principle that the sum of the moments of resistances to compression equalled those to tension*. This he corrected in his work in 1837.

Tredgold placed the neutral axis at the centre of *rectangular* sections, but did not seem to understand the simple principles of Parent and Coulomb, which would have enabled him to establish the correct position for all forms of sections.

Navier is one of the most noted authors who has written upon this subject. At first he accepted the statements of Bernoulli and Marriotte, relative to the indifference of the position of the *neutral axis*, but showed that if it be assumed at the base of the section, or at the middle of the depth, that for *flexure the moment of resistance would be as the cube of the depth, and not as the square, as in the case of rupture*. Afterwards, he established an erroneous equation on

the hypothesis that the resistance to flexure was proportional to the curvature of the fibre; and also to its relative elongation, which gave him a compound expression of two terms for the sum of the moments of resistance. But he corrected this expression in his first course to *L'école des Ponts et Chaussées*, and gave a correct expression for the resistance of the fibres when the position of the nextral axis is shown; but, like Duleau, he determined this axis by assuming that the sum of the moments of resistance to compression equaled those for tension. On the 14th of April, 1821, he presented his celebrated paper "On the Laws of Equilibrium and Movement of Solid Elastic Bodies," which was the foundation of *Molecular Mechanics*, or the general theory of elasticity. This was a thorough analytical investigation. In his course, in 1824, published in 1826, he corrected all his previous errors, and was the first to establish the principle *that the neutral axis passed through the centre of gravity of the section when the material is of uniform texture, and the strain is within the elastic limit.* In the same course he brought out more fully the distinction between the law of resistance to flexure and ultimate resistance to rupture, and solved many problems not before attempted. *He also made an essential distinction between gradual and sudden rupture;* but in regard to the latter, it is nearly or quite impossible to assign the law, and is of little practical importance. He also showed how the formulas for a limited strain can be deduced from those for flexure, and added the remark, which has since become very useful, that *it was often better, in practice, to use for safety the elastic limit than a fractional part of the resultant strength.* In the same course he solved the problem of finding the reaction of the supports when a straight piece rests on several horizontal supports and is loaded at different points; *and also the resistance of a piece fixed at both ends, and loaded at any point between the ends.* But one of the greatest steps towards making a practical use of the theoretical investigations of flexure, was made by this noted scholar, in making an approximate solution of the elastic curve. The exact expression of the elastic curve was first given by James Bernoulli, in an enigma, in 1691, and explicitly given in 1694; which is *that the radius of curvature is inversely as the moment of the force producing flexure.* This principle gives rise to a differential equation of the second order, which may generally be integrated once directly and in finite terms; but the second integration is only effected by

quadratures, or elliptic functions, or by series. But Navier assumed that $D s = d x$ for small deflections, or what is the same, *that the tangent of the angle which the curve of the neutral axis makes with the axis of x is practically unity for small deflections.* This approximation enabled him to make an easy solution of a large class of problems not before attempted. Problems of elastic curves were discussed long before the laws of elasticity were known. Galileo *supposed* that a piece slightly bent was a parabola; and P. Pardis and Laius considered it a catenary. In 1744, Euler made a complete enumeration of elastic curves, and made nine classes, in the first of which the force is nearly or quite parallel to the axis of the piece, and has given rise to much discussion as applied to columns.

M. Persey, in 1834, stated that a single equation of moments about the neutral axis is not sufficient to establish the equilibrium of the exterior and interior forces of tension and compression, unless the neutral axis is one of the principal moments of inertia passing through the centre of gravity. If the resultant of all the forces is not perpendicular to one of the principal axes, the beam will be sprung sidewise. It is then necessary to take two equations of moments of the forces, and it is most simple to take them about the principal axes. If the principle axes of a straight piece—*i. e.*, the axis of the piece is straight, but the sides warped,—are not in a plane, the piece will be twisted at the same time that it is bent.

In 1840, M. Poncelet reduced, theoretically, the danger of longitudinal compression to that of transverse dilations.

Slipping and Tangential Forces.

Beams subjected to flexure experience two other kinds of strain.

1. That which results from slipping of the transverse sections upon each other, and of the slipping, necessarily simultaneous, of the longitudinal fibres upon each other.

2. That which results from tension or the unequal rotation of the sections about the axis of the piece.

M. Vicat called attention to the former in 1833, and called it a *transverse force.* Coulomb, in 1773, gave an expression which could easily be interpreted to mean the same thing. Young spoke of detrusion. Rankine calls it *transverse shearing* and *longitudinal shearing.* The latter is easily calculated when it is evenly distributed over the surface; but it is known to diminish from the centre

to the base of the section. In the practical investigations of flexure, we assume that sections originally perpendicular to the axis of the piece remain normal to the neutral axis during flexure, and this is true only when the flexure is equal from one end to the other, or is the arc of a circle. In all other cases the sections cut the axis at a greater angle than they do the surfaces of the piece, and are warped.

Torsion.

Torsion is to the slipping of a section over its adjacent one, what flexure is to the extension or compression of the fibres. Torsion was studied for the first time by Coulomb, in 1784, and afterwards the theory was discussed by Lagrange, who gave incorrect equations, as was indicated by Binet, in 1814, and which were corrected by Poisson, in 1816 and 1833. Cauchy made an analysis of this subject, and considered the effect of warped transverse sections. His formulas formed the basis of the more thorough analysis of the subject, both theoretical and practical, by Chevandier and Wertheim, the results of which are published in several numbers of the *Annals de Chemie et Physique*. Notwithstanding the exact analytical solution of the problem of torsion is complicated, yet the exact problem of flexure by the mixed method is more complicated, and has been obtained only after long researches.—*Louisville Journal*, 1856.

Resiliance.

To determine whether the limits of cohesion when a body is subjected to a shock is reached, it is necessary to calculate what M. Poncelet has called *résistance vive*, and Young the *resiliance* of the piece or system—that is, to know the live power possessed by the system to resist the blow or shock. To do this we must know the greatest elongation $i = \frac{R_o}{E}$ beyond which it is not safe to pass, and then calculated the total dynamic work of the elastic resistance which is offered by the piece or system. The effect of the shock of a body very *resistant*, such as a spherical shell against a rod, is easily calculated when the weight of the rod is neglected; but in most practical cases the problem is a very difficult one to solve.

Molecular Mechanics.

Without any reference to the practical application of molecular mechanics, the subject has received the attention of the ablest ma-

thematicians since the days of Bernoulli. It would extend this article much beyond suitable limits, to give a sketch of the analysis upon this topic by such noted men as Boscovich, Clairaut, Laplace, Newton, Fresnel, Cauchy, and Poisson.

Their discussions of such subjects as light, heat, capillary attraction, and the like, lead to practical results; but their discussions of the resistance of solids was of little practical value until the principles of elasticity were known and recognized. Of late the conditions of stability are founded upon the limits of elasticity—that is, the strain should not be so severe as to damage the elasticity of the material; but formerly it was established upon the cohesive resistance of the particles. These principles, in some cases, give very different results. In 1855, W. J. Macquorn Rankin gave an article on potential work, strains, stresses, &c., which was published in the *Transactions of the Royal Society* for that year.

The article from which these facts are taken closes with a detailed account of the experiments which have been made upon solids from the time the subject has been treated as a science to the present time; and although reference is made to the *Civil Engineers' Journal* of 1862, yet the author has strangely ignored the theory and experiments of Barlow on the "Resistance to Flexure," published in the *Civil Engineers' Journal* for 1856 and 1858. Barlow, as well as other writers, had observed that the modulus of rupture for transverse stress was not the same as the absolute strength or tenacity of the material when ruptured by tension; and he proposed a new theory, called by him "*Resistance to Flexure*," which was intended to explain the discrepancy. The term I consider unfortunate, for all resistances to bending may be considered a *resistance to flexure*, whereas he intended to include only a certain class of strains. The stress which he considers is the same as that called by Rankine "*Longitudinal Shearing Stress*." Whether his theory be correct or not, the spirit of the article cannot be too highly commended. He presented the results of a large number of experiments, some of which were made by himself, and others were selected, and their agreement with the results as given by his theory, went far towards confirming his views. We can expect to establish the true theory of the resistance to rupture of solids under all the conditions to which they may be subjected, only after a long series of scientific experiments.

University of Michigan, December 3, 1868

Mechanics, Physics, and Chemistry.

THE BEST MODES OF TESTING THE POWER AND ECONOMY OF THE STEAM ENGINE.

By Charles E. Emery.

Late of the U. S. Navy and U. S. Steam Expansion Experiments.

(Continued from page 55.)

A good dynamometer is the only instrument that can be depended upon to accurately measure the useful work which an engine is capable of performing; still, the best instruments of this kind have many disadvantages for every-day practical use.

In the first place, especially when great power is to be measured, the dynamometer must needs be a large, heavy, and expensive measuring machine, rather than an instrument; consequently, but few can afford to purchase it. The dynamometers, at present in the market, are sold chiefly to establishments that rent rooms with power, where a small machine can be shifted about the building in the night, and so attached as next day to indicate the power used by one of the tenants.

The steam indicator, on the contrary, is neat and compact, and can be easily applied to nearly every kind of steam engine. Its use has, therefore, become so general, that it is acknowledged throughout the world as the standard measure of the power of the steam engine. We have shown the instrument defective, still we cannot point out another, fit in every respect, to take its place. We do say that the dynamometer should always be used to measure the power; but we acknowledge that, in a majority of cases, it is impracticable to apply it. Then, as we have proposed two methods of investigation, one for careful scientific experiment, and the other for practical and tolerably accurate comparison, we conclude that the first would always require the use of the dynamometer, and the latter whenever it is practicable to employ it. Generally, however, until a new instrument is perfected, we must use the indicator alone in ordinary practical trials. It should only be trusted, however, under the circumstances, and subject to the precautions we have before expressed.

When the indicated power alone is used, it is important to know the probable friction of the engine, so that the net power, or that portion available for useful work, may be estimated. A favorite method is to take an indicator friction diagram from the engine, when disconnected from its load, and running at its working speed. The mean friction pressure thus obtained is supposed to be constant at all loads. Hence it is usual to deduct from the indicated working pressure the indicated friction pressure previously obtained, when the remainder represents the force available to produce motion. From this, however, is deducted the friction of the load, usually called seven and a half per cent.; and the net power is calculated from the second remainder. For instance, if the mean working pressure be 42 pounds, and the friction pressure 2 pounds, 40 pounds is available to produce motion without a load; and seven and one half per cent. of this, or 3 pounds, represents the friction of the load; so that 5 pounds pressure is lost in friction, or about twelve per cent. of the whole. This mode of calculation cannot always be depended upon. We have known a case where the mean indicated working pressure in the cylinder was only 8 pounds, and the friction pressure 2 pounds. Consequently, by the above method, about thirty per cent. of the power was absorbed by friction; but the dynamometer showed that less than ten per cent. was lost in that way. Similar cases, differing only in extent, will be found quite frequent. The reason is, that engines are packed for the working, and not for the friction pressure. If the steam pressure be 100 pounds, the packing must embrace the piston and valve rods with sufficient force to prevent leakage, or say 105 pounds for every square inch of surface packed; and nearly the whole of this will produce friction, when a low pressure is used, but the full pressure will work in between the surfaces, and force back the packing, so that the friction from that source will be least when the engine is doing its regular duty. Spring packed pistons modify the friction in the same way. In very large engines the state of the packing would have little influence on the friction, though it certainly would seem proper to loosen the stuffing boxes before taking friction diagrams. In some cases, engines are so weakly constructed, that, though the indicator may show little friction, without a load, there will really be a great loss when the work is being done, due to parts springing out of line, etc. The dynamometer furnishes, therefore, the only true means of obtaining

the net power. In well constructed engines we should be able to calculate the friction by regarding the weight of the moving parts as part of the load, which is moving with a certain velocity in bearings of a given material, and having therefore a certain co-efficient of friction, say seven to eight per cent. For ordinary purposes, when trial is not convenient, we may assume the friction of small engines, of bad design, or of any engine with weak framing, as being from twenty to twenty-five per cent. of the indicated power: while in good engines, of ordinary shape and proportions, it is sufficient to allow fifteen per cent. for medium size, and as low as ten per cent. or even eight per cent, in exceptional cases, in large engines of solid construction and good workmanship.

Having described the instruments used in determining the power of the steam engine, we propose to postpone future remarks upon the proper methods of their application and use, until the closing general discussions; and we will now proceed with the next branch of inquiry; namely:

II. The Economy or Cost of the Power.

Money is the standard unit of value. Hence, everything which costs money, that is required in order to obtain the steam power in any case, is a proper charge to the cost of the power. Therefore, strictly speaking, the cost of the fuel, of the oil, and of needed repairs, together with the wages of the attendants, and also, perhaps, a sinking fund for prospective renewals, should all form part of the aggregate cost. Nor should either of these items be neglected. It would be poor economy for a person to purchase an engine designed to save fuel, which, for any reason was liable to frequent derangement; for it is not alone the cost of the repairs which are to be considered, but the losses which occur from stopping work in the mill or factory. We cannot, however, in our present inquiry, discuss matters of design (though they should always be considered by a purchaser), but must confine ourselves to the methods and means employed to ascertain the economy of fuel.

The combustion of the fuel evolves heat, which uses water as a vehicle, and is carried with it to the engine, and there produces the power. The true measure of the cost, then, is the quantity of heat required to perform a certain quantity of work. Heat being imponderable, can be measured only by its effects on other bodies. The standard unit of heat, or "heat unit," is the heat required to

raise the temperature of one pound of distilled water at 39° one degree Fahrenheit. The mechanical equivalent of a unit of heat is 772 foot-pounds of work; but the best steam engines obtain only about one-tenth of that quantity. Such a result has often been regretted by scientific minds, and many have spoken of it as mysterious. We consider the steam engine of to-day very defective. Some of the defects are inherent; they can be pointed out, but cannot be remedied without changing the general principles of construction. The majority of the practical loss has, however, never been satisfactorily explained. The writer, like others, has his own theories on the subject, but he has no desire to present them publicly till they have been tested; for if they be correct, the principal difficulties can be removed. Few appreciate the extent of the losses in the steam engine. It is only the best examples that utilize even one-tenth of the heat. In such cases, one-tenth is condensed for the work, and about four-tenths is wasted in the clearances and the exhausting steam, even when expansion is carried on, until the terminal equals the back pressure. The remaining five-tenths are imperfectly accounted for. Cases are not unfrequent where only three to five per cent. of the heat taken from the boiler is utilized in work. The discrepancies occur chiefly at the higher grades of expansion. Without expansion, it is easy to understand that most of the heat must go away with the exhaust.

When steam is generated by the application of heat in the boiler, to water only, the water, in becoming steam, always takes up a certain fixed quantity of heat; in other words, becomes saturated with it, and forms saturated steam. Hence, if we can measure the water evaporated, to produce the power of an engine, we can easily estimate the quantity of heat used. The feed water is therefore a perfect measure of the comparative cost of the power, when evaporated in a good boiler, having no superheating surface. The economy of steam machinery is, however, generally measured by the amount of coal or other fuel consumed to perform a certain quantity of work. The conventional standard of comparison between all kinds of engines is, the number of pounds of coal burned per indicated horse-power per hour. The indicated power can be obtained with comparative ease, as has been explained; so also can the coal per hour. Hence the above standard has the merit of great simplicity, and consequently is used by all nations. We must therefore adopt it, or at least use it, in order to be able to compare our results with

those of others; still the method is liable to very considerable errors, which we will examine with the view of correcting them.

It has been shown that the indicator cannot always be relied upon to accurately measure the power. The qualities of coal vary so much, also, in different localities, that the amount consumed does not furnish an accurate comparative measure of the cost of the power. When the coal measure alone is used, too, the engines and boilers are both tested together, which gives no opportunity to ascertain which of the two is entitled to the credit of the performance. This standard will not then answer the purpose of a scientific investigation. In such case, we must ascertain, in addition to the coal, the amount of water evaporated; we can then estimate the value of the coal, and the separate efficiency of both the engine and boiler. The value of the coal, and the efficiency of the boiler, are shown by the number of pounds of water evaporated per pound of coal, and the economy of the engine as compared with that of others by calculating the number of pounds of steam used per horse-power per hour. The weight of the steam used is, of course, the same as that of the water evaporated.

In all ordinary practical trials, the economy must be determined simply by the quantity of fuel consumed to produce the power. Hence, we will first try and find a solution of the difficulties which attend this kind of measurement.

The Fuel.

The different kinds of fuel vary so much in value that it is impossible to accurately compare them. Coal being most generally used, is the natural standard: but there are so many varieties of this necessary article, varying greatly in quality, that it seems a hopeless task to try and compare the performance of steam engines in different parts of the world, or even of our own country, by the consumption of differing coal, which may vary twenty per cent. in heat-producing power. The best way is, evidently, in comparative trials, to use selected coal from the same mine. Yet, how rarely can this be done! and even if this precaution be taken in certain cases, how can a comparison be made with the results obtained by others widely separated, and possessing, possibly, differing views? We must say that the problem cannot be solved with scientific accuracy; still we are able to suggest some corrections which will

reduce all varieties of good coal to substantially the same standard, and thus enable us to use this measure in simple practical trials.

We cannot examine in this paper, with any minuteness, the chemical constituents of the different varieties of coal. For our purpose we will simply divide them into two portions: namely, the non-combustible and combustible.

The non-combustible portion consists, for the most part, of earthy matters, though oxygen and nitrogen gases are often present; and most coals absorb considerable water. The combustible portion consists of carbon and hydrogen, the first largely predominating. In American anthracite about three per cent. of the combustible is hydrogen. The semi-anthracite combustible contains about five per cent.; and the bituminous varieties a larger proportion, varying with the locality of the mines. It is authoritatively stated, that in some varieties of Ohio and West Pennsylvania coal, the hydrogen element is often twenty-four per cent. of the whole combustible. For the consumption of equal weights of hydrogen and carbon, the first requires three times as much oxygen as the latter: the heat resulting should therefore bear a somewhat similar proportion. Favre, Silberman, Andrews, and others, have, from experiment, estimated the calorific value of one pound of carbon to be the heating of about 14,000 pounds of water, one degree Fahrenheit. The corresponding value of hydrogen was similarly determined to be about 60,000 heat units. Bituminous coal, containing considerable hydrogen, should therefore produce very much more heat in combustion than anthracite; but in practice the difference is comparatively small. Mere differences in mechanical structure appear to have a greater influence than chemical constitution. The reason is not evident. The latent heat of the steam resulting from the combustion of hydrogen, which is lost in the atmosphere, will not nearly account for the discrepancy. Without attempting an explanation, except perhaps imperfect combustion, we can, for our purpose, only turn to the records of practical experiments, and find what different kinds of coal have done, and may therefore be expected to do again.

Bourne gives the evaporation efficiency of thirty varieties of coal from different parts of the British Isles, or from 7 to 10·2 pounds of water from a temperature of 212°. The average was 8·7 pounds. These coals are, as is well known, of the soft or bituminous variety. The results of experiments made by the Navy Department, with

thirteen varieties of American anthracite, from different parts of the Pennsylvania coal field, gave a mean evaporative efficiency per pound of coal of 8·9 pounds of water, from a temperature of 212° Fahrenheit. Three specimens of American bituminous coal gave a mean result of 9·9 pounds, under similar conditions. The figures make it appear that our American coals are superior to those of other nations. Professor Johnson, at an earlier period, made some experiments for our Government, with smaller quantities, but obtained more marked results in the same direction. On the contrary, the engineers of the English and French steamers, out of this port, speak of our Cumberland and kindred varieties of coal as inferior to those procured at home. We are in search of the truth, and cannot therefore cater to national vanity. Our best bituminous and clean, free-burning anthracite coals are undoubtedly better than can be found in large quantities in any other part of the globe. All must admit, however, that some of our American bituminous coals are almost identical with the English in appearance and chemical constitution. Both should therefore give the same results, when tested under the same circumstances. In the experiments above mentioned, the English coals comprised a greater number of kinds, the bad being averaged with the good. The United States government experiments were tried with the greatest care, and in a boiler better proportioned for economy, probably, than the average in England. On the whole, we think it fair to assume that the English and American bituminous coals, of the qualities ordinarily supplied to the market, are substantially equal in value, though selected varieties, fresh from our mines, would of course give much better results.

The Government experiments above mentioned showed that the evaporative efficiency of the American anthracite, and the American bituminous coals are in the proportion of 8·9 to 9·9.

The anthracite as a rule, contains much more refuse than the other varieties. The English coals probably average ten per cent. of waste; the West Pennsylvania and Ohio coals have only five per cent., and the maximum of our bituminous coals rarely exceeds thirteen per cent. On the contrary, the refuse from anthracite rarely falls as low as ten per cent., and often reaches to twenty-four per cent., so that, on the average, its waste is double that of the bituminous varieties. It will therefore be interesting for us to examine the results produced by the combustible portions of the different

kinds of coal. The part consumed is called the "combustible," and is found by deducting from the weight of the coal the weight of the ashes, clinkers, soot, etc., which can be collected after the trial. Referring again to the Navy experiments, we find that the mean evaporative efficiency of thirteen varieties of American anthracite combustible was equal to the evaporation of 10·69 pounds of water, from a temperature of 212°, and, for the three varieties of bituminous combustible, the corresponding effect was 10·84 pounds. The results are practically identical. By throwing out of the comparison some of the varieties of anthracite, which justly have a poor reputation in the market, the preponderance would be upon the other side. If, then, we take it for granted that the average foreign and American and bituminous coals are substantially equal in value, the value of the combustible of the foreign coal will equal that of American bituminous and American anthracite, and we may assume that the combustible of the coal, burned in any case, is a tolerably accurate comparative measure of the economy of a steam engine. All these restrictive qualifications are necessary, for if selected coal of the best quality, be used in a trial, the results will be above the average in any case. We wish simply to indicate that the greatest difference in the results given by different coals is due to the difference in the quantity of non-combustible matter, so that, if this be thrown out, the weight of the combustible remaining gives the nearest approach possible, without absolute trial, to the comparative heat-producing powers of different specimens. The best standard to show the comparative economy of the steam engine, other than that of the steam used, is therefore "The number of pounds of combustible used per horse-power per hour."

We cannot, fairly, however, compare the combustible per horse-power per hour, used in experiments here, with other experiments where only the coal was noted. This necessitates us to correct the amount of coal used by a common standard, founded on the combustible. Good bituminous coals, here and in England, have about ten per cent. refuse; hence, to make our experiments compare with those abroad, as well as for convenience, we suggest that in every case, the coal burned in determining the economy of a steam engine be reduced to a common standard of ten per cent. refuse. Let us see the effect of this. The true comparative test for engines is the amount of heat they receive; we have shown that the heat-producing power of the coal is proportioned to the weight of the com-

bustible; hence, if the weight of the coal be also proportioned to that of the combustible, it also expresses the relative economy. The coal is so proportioned when it leaves the same per centage of refuse, so by our plan of correcting the weight of the coal by its combustible, so as to give ten per cent. refuse in each case, the weight of the coal is a true comparative test of the relative economy of the engine. For instance, 100 pounds of coal leaving twenty per cent. refuse will evaporate no more water than 88·9 pounds leaving ten per cent. refuse, for both contain only 80 pounds of combustible. If to the combustible we add one-ninth of its weight, the quantity added is one-tenth, or ten per cent. of the sum, which represents the weight of the coal, corrected to the uniform standard of ten per cent. refuse. Suppose a horse-power in a certain foreign steamship costs 2·8 pounds of bituminous coal per hour, and in an American vessel it costs three pounds of coal, using anthracite, are we to say our engines are inferior? Let us see. We first deduct the refuse from the anthracite—for instance, twenty per cent., which leaves 2·4 pounds of combustible. This, then, is nine-tenths of the weight of coal having ten per cent. of refuse; so multiply 2·4 by $\frac{10}{9}$, gives 2·67 pounds as the true cost of the power in the American engine, to compare with 2·8 pounds used by the foreigner, when both are compared by the same standard.

We have been thus explicit, because the fuel is so generally used in the comparison of the performance of steam engines. The coal bills of course show the absolute cost of the power in any particular case, no matter what quality of coal was used; but, under such circumstances, the weight of coal consumed, even when corrected as above pointed out, is, as must be seen, but an imperfect *comparative* measure. To make comparisons sufficiently correct to answer the demands of science, we must measure the steam used in each case—in other words, compare engines by the number of pounds of steam used per horse-power per hour.

The calculations are usually made from the pressure shown at the termination of the stroke; the assumption being that the engine uses, at every stroke, one full cylinder of steam at that pressure. In other cases, however, the initial pressure, and the portion of the cylinder filled at the point of cut-off, are used in the calculation. These methods of determination pre-suppose that dry or saturated steam enters the cylinder, which may be true, and that the steam continues in this state, through at least part of the stroke, without

condensation, which is never the case. Steam is necessarily condensed to set free the heat transmuted into the work done; and the temperature of the metal of the cylinder is a mean of the temperatures to which it is subjected, and therefore the surfaces form a condenser with respect to the initial steam. The consequence is, that there is always more steam taken from the boiler than is shown by the indicator; the discrepancy increasing with the degree of expansion and amount of external refrigeration. Clarke, in his work on the locomotive, points out great differences between the amount of steam calculated from the initial and terminal pressures shown by the indicator; and yet uses the first in all his calculations. Later experiments, where the steam has been actually measured, show that in small engines twenty to thirty per cent. of the steam is unaccounted for by the indicator at full stroke; and as high as sixty to eighty per cent. when the steam is expanded considerably. Large engines show a small discrepancy at full stroke, which rises to thirty, and often fifty per cent., with shorter admissions. The best examples of the English double cylinder pumping engines with steam-jacketed cylinders use thirty-three per cent. more steam than is shown by the indicator on the cylinders. This method of determination is therefore absolutely worthless for our purpose, as it furnishes no basis for reliable comparative tests. These discrepancies show us where a great loss takes place in the use of the steam engine. They have been ascertained, in practice, by indicating the engine and measuring the water pumped into the boiler, and evaporated there, to furnish steam. In other cases, the exhaust steam of the engine has, by surface condensation, been reduced to water, and its quantity determined by measuring or weighing it. The weight of feed-water, or what is the same thing, of steam used in any case, to produce a given power, may, by either of these plans be ascertained with scrupulous accuracy; and if the coal be weighed at the same time, the evaporative efficiency of the boiler can also be determined, and the excellence of both engine and boiler be detected and credited aright.

In addition to the standards above given, expressing the economy of the engine, others of special application are used, which give the cost in terms of that for which money is paid, namely, the coal, and the result in that which returns the money. For instance, the miller speaks of the number of pounds of coal it requires to grind a barrel of flour—a thing, by the way, which may depend as much

upon the condition of the mill as of the steam machinery. Locomotives are rated by the number of pounds of coal or coke burned per ton per mile. So, also, what is known as the "duty" of a pumping engine, is the number of foot-pounds of work derived from the consumption of a certain quantity of coal.

(To be continued.)

SPECTACLE GLASSES FOR PUBLIC SPEAKERS.

By S. W. Robinson.

It has been set forth by John B. Gough, the great lecturer, that a public speaker should wear nothing that will throw flashes. This truth appears self-evident on enunciation, and should be particularly regarded by all public speakers. A glistening button, watch-chain pendant, or breast-pin is always noticed. The richest ornaments, particularly those set with diamond jewels, are worst of all. But what can be more effective in projecting flashes than spectacle glasses as now constructed, especially those of slight convexity, such as should be adopted by persons beginning to wear them? First, perhaps, we see the natural eyes of the speaker; then suddenly, two fire-balls take their place, of demon-like appearance, winking alternately, or in concert, to the turn of the head. The object in view in the few words now offered, is to propose a remedy, or a partial one at least, for this inconveniency regarding spectacle glasses.

Light reflected from a plane surface, as large as a spectacle glass, proceeding from the blaze of a gas burner, will appear to an observer, as far from the glass as the glass is from the burner, to proceed from the whole surface of the glass when the position of the latter is favorable; because, under these circumstances, it is only necessary that the blaze of the burner should have, as it very nearly does, twice the breadth of the glass. If the surfaces of the glass be slightly curved, it is only necessary that the burner be comparatively a little nearer to it. The effect is still worse when the light reflected, proceeds from the broad surface of a window; and the spectacle lens may then appear fully illuminated with considerable convexity. This not only produces an annoying flash, but ever and anon perfectly obscures the eyes of the speaker wearing the spectacles. But the greater the convexity of the lenses of the spectacles, the smaller will the reflected image of any given object appear. If this be carried to a sufficient extent, the images

of a whole window even may be reduced to such an extent as to cause no serious inconvenience, any more than the images of the same objects as ordinarily reflected from the cornea of the eye itself. To secure the desired degree of convexity of the two surfaces of the lenses of the spectacles, without adding to the magnifying power, it will be necessary to grind the lenses in the form of the meniscus. This will require some additional trouble on the part of the practical optician; and such spectacles would consequently be more costly. But cannot, and would not many of our public speakers, such as clergymen and lecturers, avail themselves of the use of such glasses if they could be had, even at a considerably extra expense. Let opticians give their attention to the production of a few such spectacles, if for no other purpose than to try the experiment, believing that if they can be produced without a greater sacrifice than the object gained, that they will thus become contributors to the relief of a present serious want.

This form of spectacle will also possess some incidental advantages. When the eyes are turned considerably to one side or the other, the rays of light will reach them by a more direct passage through the substance of the glass, and thus the eye receive more light if the glass is not perfectly pure and clear. The frame of such glasses can also be worn nearer to the eyes, and give the same amount of room in front of them, so as to admit of a broader range of lateral vision without interference with the frames of the lenses, or passing outside of them altogether.

It would seem that the eyes, thus favored, with a clear and unobstructed view, could almost believe themselves enjoying youthful days.

University of Michigan.

COMPOSITION AND RESOLUTION OF FORCES.

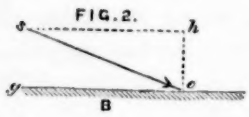
By John C. Trautwine, C. E.

The following illustration of the composition and resolution of forces, is familiar to everybody; but it appears to me that the deductions that are drawn from it by even eminent authorites, are *entirely erroneous*. Let *a b c d*, Fig. 1, represent a floating cake of ice. Upon it a man starts from *a* to walk to *b*, on the opposite shore; but during the same time,

FIG. 1.

the ice floats from *a* to *c;* so that although actually walking along only the line *a b*, he arrives at *d*, instead of at *b;* having really traversed the line *a d*. Now the line *a b* may be taken to represent the force which he expends upon himself during his walk; and *a c* that expended upon him by the ice in the same time. The savans tell us that since he has actually crossed the stream, which is supposed to be as wide at *c d* as it is at *a b*, no part of the force *a b* can have been lost; and again, since the ice has carried him as far down the stream as it has itself moved down, therefore no part of the force *a c* has been lost. Hence, they say the resultant *a d* actually represents the united wholes of *a b* and *a c;* and this is accounted for on their beautiful (ridiculous?) principle of "the independence of the simultaneous action of many forces upon the same point;" from which says Morin* "it follows *quite naturally* (the italics are my own), that the forces which produce these motions, exert actions independent of each other. Some compare this independence to the crossings of different circles of little waves; as if these did not affect each other. My own impression is, that so far from being *independent* of each other, they actually (not only in this case, but in all others), in a great measure *destroy* each other; and that it is only their *remains* that constitute the resultant force, *a d*. The force *a b* may be conceived (as is perfectly well known to the veriest tyro in mechanics), to be made up of two forces respectively, equal to *a o* and *o b;* and the force *a c* in like manner, of two equal to *a i* and *i c*. When the forces *a b* and *a c*, come into collision in the man, the two subcomponents *o b* and *i c*, cannot take any part in *moving* him; for being equal, and in diametrically opposite directions, they react against, counterbalance, or render null, each other's action; they become *static* force. They *can do* nothing more; and they *must do* that.

If to the body B, Fig. 2, we apply an oblique force, *s o*, (of which the components are *g o* equal to *s h;* and *h o*,) we know that the component *h o*, at right angles to the surface *g o*, encounters the equal opposing force of B; against this it reacts; and is thereby rendered static, or incapable of producing motion. But the component *g o*, equal to *s h*, still remains as moving force.

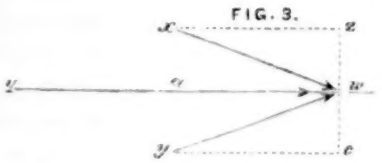


In the same manner, if we apply two forces, *x w* and *y w*, Fig.

* See Bennett's Translation, page 132,

3, to a man, or to any other body, *w*, whether on floating ice or on land (unless there is some mysterious counteracting principle in floating ice, which I am unable to comprehend), the two sub-components, *z w*, *c w*, will become static; and only the two which are equal to *x z* and *y c; or to the resultant, q w;* will remain as dynamic or moving force, along the diagonal, of the parallelogram of forces, to move the man. Depend upon it, the principle of the "independence of the simultaneous action of two or more forces upon the same point," (even as applied to the heavenly bodies) is a fallacy; although Morin, page 119, cites a most delicate and conclusive experiment by M. Tresca, sub-director of the Conservatoire, in support of it. A resultant is not a measure of the forces represented by the components; it is only a measure of its own force; which is always less than those represented by its components. In statics it represents a force, which if reversed in direction, so as to become an anti-resultant, will balance the *remains* of the components; and in dynamics, it represents the smaller moving force left in the components after they have partly rendered each other static. No *practical* error, however, will arise from saying as usual in all cases, that the resultant is a force which, if reversed in direction, will *balance its components*. That an error *in science*, however, has arisen, is I think clearly shown by the necessity for inventing the "independence" theory, to explain away the supposed mystery, alias adopted fallacy.

FIG. 3.

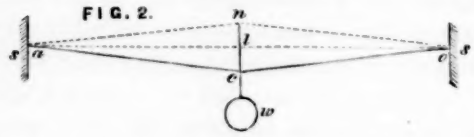


Since the resultant, so-called, of two oblique moving forces, is in fact the result of their *remains* only, it is plain that the components can in no way, whatever, be regarded as representing either the resultant, or its effects. For although two components *diminish each other* in producing the resultant; the resultant *cannot increase itself* so as to produce the components. Two oblique moving forces, which we will call 10 and 20, may so diminish each other by reaction, or straining against each other, in their efforts to change each other directions, that their remains, resultant, or diagonal force, will impart to a body a motion of but 5. But if that motion (or rather that moving force of 5 which produces that motion), be reversed, or turned back on its course, it plainly cannot impart a motion of 10 to one-half of the body; and a motion of 20 to the other half.

Whatever the astronomers and other savans may say, *force* IS *always lost in changing the* DIRECTION *of force;* and the two components represent two moving forces to which the resultant would be equal, *if it was large enough**

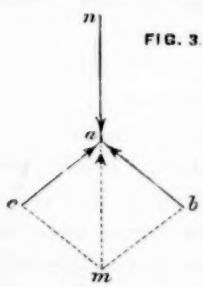
But it is intimated at times that a resultant actually *does increase itself;* thus we are told that if at the centre, *c*, of the rope, *a c o*, Fig. 2, attached to two walls, *s s*, we apply a weight, or force of gravity, *w*, of one ton; represented by the resultant, or diagonal, *n c;* the said force of one ton will produce two forces, *a c*, *o c*, each (per figure) of four and a half tons; or together, nine times as great as the resultant force, *n c*, itself, which produces them. But this is altogether a mistake; for the fact is, that the one ton has nothing to do with producing these nine tons; they are produced entirely by the walls. The force, *a c*, is the resultant of *a l*, and one-half of *n c;* and *o c* in like manner, of *o l* and the other half of *n c;* and all these are derived from the walls *exclusively*. The one ton contributes an entirely distinct vertical force only equal to one ton.

FIG. 2.



I will endeavor to make this clearer; and to show that force, whether as *motion* or *strain*, cannot produce components greater than itself. I will premise that I call a force, *n a*, Fig. 3, which is equal to a resultant, *m a;* and diametrically opposed to it in direction, an *anti-resultant*. Now let *c a* and *b a* be two *moving* forces; *m a* their resultant; and *n a* their anti-resultant. Taking it for granted that the reader already sees that although *c a* and *b a* may partially destroy each other, so that the resulting motion would only be one from *a* to *n;* and also that a motion from *n* to *a*, could not reproduce the destroyed parts, so as to restore two forces, *a c* and *a b;* I will proceed to *straining* (pulling or pushing, or reacting) forces. Let *c a*, *b a* push against *a*. Their resultant, or their remains, is a push equal to *m a*. Now, will an anti-resultant push, *n a*, produce the two pushes, *c a*,

FIG. 3.



* A resultant has been defined to be a single force, which if reversed in direction, would balance *its components;* it should be *the remains* of its components: for a resultant if reversed can balance no forces amounting to more than itself.

b a? The reader will probably say yes. I say no. *We, ourselves, must absolutely provide forces in their proper positions, before n a will push at all;* and we must provide at least as much as *c a*, *b a* lost in producing *m a*. I repeat, *n a* cannot push at *a* at all, unless *we* first put forces there for it to push against; it cannot make forces for itself to push against. When we apply two oblique forces to produce *motion*, we must take care that there is no third force present to prevent said motion; but when we apply them to produce *strain*, we must also apply a third force for them to strain *against.*

All this will probably be better understood after reading what is said in reference to Fig. 5.

There is a prevailing idea that one force, however great, tending in one direction, can oppose no resistance to another force, however small, tending to move it at *right angles* to that direction. This is a palpable error. The doctrine is supposed to be confirmed by such illustrations as the following:

If a rigid (unchangeable) body weighing but an ounce, be squeezed horizontally between the jaws of a vise, with any amount of force whatever, say, 1,000 tons, then this immense *horizontal* force will not tend in the slightest degree to prevent the *vertical* one ounce of gravity from moving the one ounce body downwards, from between the jaws of the vise. It is true that the body will not so move; but its motion is prevented by the reaction upwards of one ounce of a third force, *friction;* which is generated by the pressure of the vise.

Now, this is all very correct; but unfortunately it has no connection whatever with the subject; for the vise presents no force tending to move the body horizontally, or at right angles to its vertical direction. On the contrary, it presents *two equal and opposite forces;* either one of which *would* move the body horizontally were it not that the other tends *equally* to move it horizontally *in the opposite direction.* These two tendencies to motion, react against, strain, or destroy each other against the internal cohesive *forces* of the particles which compose the body; but upon the *matter of the rigid body itself*, that is, upon *the particles themselves, they produce no effect whatever.*

If the body is not rigid, and if the cohesive forces of the particles are not sufficiently great to withstand the reacting forces of the vise, then the particles will be displaced; the body will be reduced

to fragments, and the overplus of the reacting forces of the vise having no longer any opposing forces to *strain* against, will be converted into *moving* forces, and will set the fragments into motion, as so many separate bodies, in all directions.

I suggest the following as a safe principle. If it has been before laid down, I have not seen it, namely: that reacting forces cannot affect a moving one; and, vice versa, that a moving one cannot affect reacting ones. The non-recognition of this broad principle, has, in my opinion, been the cause of much error, not only among practical men, but among some authorities on such subjects. I will merely offer one homely illustration, the simplicity of which is better calculated to impress the fact upon the memory, than one drawn from a more dignified source would be. Suppose the vise and the squeezed ball between its jaws to be let fall together. It is plain that the squeezing or reacting forces of the vise will not cause the moving force of the ball to become either faster or slower; and equally plain that the downward moving force of the ball will not cause the reacting forces of the vise to squeeze it either more or less. I would direct attention to a point which appears to have escaped the notice of writers. When one, as *g d*, Fig. 4, of two oblique moving forces, *g d*, *h d*, applies at one point, *d*, forms an angle, *g d m*, with the diagonal, *m d*, greater than 90°, or a right angle, then lines *g y*, *h t*, cannot be drawn from the angles *g* and *h*, at right angles to the diagonal itself; but the diagonal must be extended equally both ways, as to *y* and *t*, for that purpose; and when this is the case, the lines *g y* and *h t* no longer measure those portions of the two forces which react against each other, and against the internal cohesive forces of the body; but *g y* and *y d* on one side, and *h t* and *t m* on the other, thus react; leaving, as before, only the diagonal or resultant, *m d*, as the measure of the *remains* applicable as motion, to moving the body, *d*, itself, as a whole. Hence, in such cases, the loss which each force undergoes, is no longer in proportion to the sine of the angle which said force forms with the resultant. Thus if *g d h m* be the shape of the cake of ice, instead of Fig. 1, and if the man walk the line *d g*, while the ice moves from *d* to *h*; then the man's own force from *d* to *g* (composed of *d y* and *y g*), would be reacted against

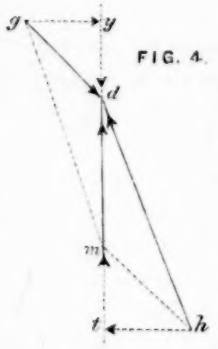


FIG. 4.

and entirely destroyed (so far as regards the man as a whole), by $m\,t$ and $t\,h$, of the force $d\,h$ of the ice; and the man would be found at m; having been carried there by $d\,m$, the remains of $d\,h$; and in fact of all the motion that remains from both the forces; the other portions having acted as *strain* against *each other in* the man.

In Fig. 5, let S and R be two rafters, sustaining a load, L, represented by the resultant, or diagonal, O g. Now, here there is no motion; what effects does this load or vertical force produce upon the rafters? We are told that L is supported by the rafters; and that it produces in them the strains, S and R. But this is incorrect in two particulars; for it is in fact supported by the walls; and, moreover, one force cannot produce strains. We will omit the consideration of the weights of the rafters themselves; or will assume them to be without weight; and then the case becomes one of three straining forces represented by R S, and O g, all applied at once at the point, O. The force along S is furnished by the wall, M; that along R, by the wall, N; and that in the direction from O to g, by the load, L. The rafters are merely mediums for conveying the forces M and N from the wall to O, as the rope conveys that of L to O. These forces of course respectively pass through the entire lengths of these mediums, each of which is consequently strained from end to end.

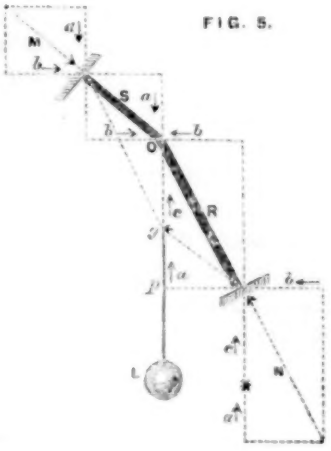


FIG. 5.

The force, M, may be supposed to be the resultant of the vertical and horizontal components, $a\,\downarrow$ g and $b\,\rightarrow$; and the force, N, of $a\,\uparrow$, $c\,\uparrow$, and $b\,\leftarrow$; and these, together with the vertical force, O g,* may all be considered to be applied to the point, O, as in the figure.

This being done, we see that $a\,\downarrow$ and $b\,\rightarrow$ of the force of the wall, M, are reacted against or balanced by $a\,\uparrow$ and $b\,\leftarrow$ of the force, of the wall, N; while the vertical, O g, is reacted against or balanced

* The force Og being already vertical, cannot be resolved into two components, one of which shall be vertical (or in its own direction); and the other in another direction. In other words, the only component a force can have *in its own direction*, is itself.

by c † of the force, N. Therefore, the three forces, S, R, and Og, precisely balance each other. I ask particular attention to the strain measured by the vertical line, $p\,o$, when one of the forces, as S, forms with the diagonal, O g, an angle, S O g, *greater than* 90°. Since this is one of the components of the strain along R, it shows that the rafter, R, actually transfers to the wall, N, *a vertical pressure greater than the weight of the entire load*, L, which is represented by the part g O only, of p O. At first sight, the possibility of this will probably be denied by the practical reader, who, deceived by the phraseology frequently employed in reference to the parallelogram of forces, will exclaim that a load cannot possibly produce a vertical pressure greater than itself. That is true; and the fact that here IS a vertical pressure greater than the load itself, is conclusive proof that it is furnished by the *walls*. The excess, $p\,g$ of the strain, p O, over or beyond g O, is caused by the two reacting forces, a ↓ and a †, of the walls, M and N. I have never seen this fact clearly laid down in any book.

ON THE NATURE AND DISTRIBUTION OF GOLD IN METALLIC SULPHIDES.

(A paper read before the Polytechnic Association in New York, December 24, 1868.)

By Dr. Adolph Ott.

It is a common belief that wherever gold is found in pyrites, the same is not contained in it in a metallic state, but combined with sulphur, or, according to others, with arsenic or antimony. This view is most generally supported by the fact that, in desulphurizing auriferous pyrites or exposing them to the action of the atmosphere, a larger quantity of the precious metal is generally obtained than if the ore is directly washed or amalgamated; and it furthermore is supported by the fact that the gold, in most cases, only becomes visible after desulphurization.

Though none but these facts are before us, which would substantiate this view, the theory of the gold being present as a sulphide, gained recently, nevertheless, a large number of adherents by the discovery of a mineral consisting chiefly of the sulphides of osmium and ruthenium. The same is found in Borneo, and has the appearance of crystalline iron. It was urged that if these

metals, which so much resemble gold in their chemical actions, occur in combinations with sulphur, it may be equally the case with gold. It was, however, not taken into account that though the existence of a natural sulphide of gold is not beyond the limits of possibility, it does not follow from the occurrence of that newly discovered sulphide, that a sulphide of gold is also present in the pyrites.

Adherents to the sulphide-theory are not only most of the miners and metallurgists of America, but also authorities like Dumas, Brogniart, Erdmann, and, in this country, Emmons, Lieber, Renwick, and Stevens, savants who are more or less known by their geological researches. Dumas, in behalf of his views, quotes the above mentioned observations, and states the fact that, by the aid of the microscope, he was unable to discern metallic gold in finely crushed auriferous ores; and also expresses the opinion that the gold, because of its electro-negative nature, must mostly be present as a compound sulphide.* So does Brogniart. Erdmann gives to the auriferous arsenical ore of Reichenstein, in Silesia, the formula: $2\,(An^2\,S^3)$, $3\,(As^2\,Sb^2\,S^3)$;† but upon what ground he bases this formula is unknown to us.

Without entering here into the profound researches of Professor Henry Würtz on on the "Genesis of Gold," where this savant defends the opinion held by us, that this metal can only be mechanically diffused in its ores, we shall consider some facts which are before very one.

As concerns the circumstance mentioned by the adherents of the sulphide-theory that more gold is obtained by first roasting the ore, instead of amalgamating it directly, it may be remarked, that this cannot be considered as a proof of the non-metallic nature of the gold in the sulphides; for, if we consider that this metal is generally contained therein in a finely-divided state, and that by crushing the ore to an ordinary fineness, a large portion thereof will always be imbedded in particles of pyrites, the above fact is easily explained. We obtain, moreover, by a repeated process of crushing and amalgamation, always a new portion of the precious metal; and in some not very rare cases, a yield is obtained by this treatment, which comes very near to that of the assay. The same results have been obtained by the washing process. We also would

* Muspratt's Chemistry, Vol. II., p. 264.

† Bruno Kerl, die Rammelsberger Huttenprocesse, p. 4.

refer to the fact that, by the amalgamation of the quartz, as well as of the pyriteous ores, rarely more than 70 per cent. of the precious metal is obtained. Prof. B. Silliman, in one instance, was unable to extract from California quartz, without the use of sodium amalgam, more than 60 per cent., and, in another not 40 per cent.*

Gold which is not visible by the unassisted eye, often not only occurs in sulphides, but also in quartz. In carefully treating it, such ore generally pays very well, while veins with auriferous ores, which furnish fine specimens for mineralogical cabinets or stock companies, are, on the average, not very profitable.

Again there is another circumstance which may sometimes contribute to the fact that a preliminary roasting of the pyrites proves more satisfactory than direct washing or amalgamation. In roasting finely-crushed auriferous pyrites from Virginia, Prof. Silliman has found that the gold which was hardly visible by the unassisted eye, was generally deposited at the top of the ore. In consequence of this, it was easily washed away, while the black sand which accompanied the quartz generally settled at the bottom. Silliman ascribes this property to the nature of the gold; while the latter existed in flattened particles, the black sand, titanium(?) occurred in angular pieces.† In such cases, *i. e.*, where the gold is met with in foliated particles, it is not quite improbable that in roasting the ore they melt, and then, overcoming the adhesion by which they floated on the top, sink to the bottom. We know well that the temperature in the desulphurization of auriferous ores is high enough to effect a volatilization of the precious metal. In any case, it is certain, that by the high temperature disintegration takes place, and thus an increase of the difference between the specific weight of the ore and that of the gold is effected.

As regards the second argument advanced by the adherents of the sulphide theory, *i. e.*, that the gold only becomes visible after a preliminary roasting, it finds its explanation in the disintegration which the pyriteous particles that surround the gold suffer by the heat. The hypothesis of a gold compound appears, therefore, quite unnecessary. Metallic gold is, besides this, sometimes visible in undecomposed pyrites with and without the assistance of a magnifying glass. In disintegrated pyrites, it often appears in

* *Chemical News*, 1866, Vol. XIV., p. 170.

† *American Journal of Science and Arts*, Vol. XXXII. p. 98.

the forms of scales and grains, which could not be the case if it was contained therein in a chemically combined state. This has also been observed when the the sulphides are dissolved in nitric acid, which fact was already known in the beginning of this century. Joseph Black, in his "Lectures on Chemistry," Vol. III., p. 384, mentions the following:—

"When the Hungarian pyrites is dissolved with aqua fortis, it is said that the gold is left, by that acid, in the form of minute atoms and fine films, which are in a metallic state. And as this pyrites varies much in the quantity of gold it contains—and some of it does not contain any—there is reason to believe that all the gold in it is metallic and pure, and only dispersed through it in very minute films not mineralized."

We are so fortunate as to meet with the following passage in Foureroy's "General System of Chemical Knowledge," Vol. VI., p. 489, which was published in 1804.

Bergmann (1735—84) observes "according to the examination of several auriferous pyrites, that the gold which is extracted from them by digestion in nitric acid, is in small angular grains, which prove that this metal existed in the state of simple mixture, and not of composition in the pyrites. Thus the ores of copper, silver, lead, iron, cobalt and antimony, from which gold is frequently extracted by docimastic and metallurgical operations, appear to contain this metal simply disseminated amongst their particles; and there is reason to believe that when these ores are decomposed, effloresce, become sulphatized and dissolved, the gold which separates from them is then carried away by the waters and deposited in the sand as if it were native gold."

In treating auriferous sulphides of Colorado, which, to the magnifying glass, did not reveal a particle of gold, Prof. Torrey of the U.S. Assay Office, has also shown that, by treating the ore with pure nitric acid, the gold appears under the microscope in laminæ, and in filiform and spongy particles.

If, finally, we consider that the gold occurs in nature in combination with rare metals, as with tellurium and silver in the sylvanite, and alloyed with mercury and bismuth, but not combined with sulphur, we necessarily must refer the hypothesis of the "gold sulphide in the pyrites" in the realms of phantasy.

It is well known that we seldom meet with a single sulphide in nature: they generally appear associated together; galena, for

instance, often occurs with zinc blende, copper and arsenical pyrites, with iron pyrites, &c.

The distribution of ores in veins being a very different one, it seems that this is also the case with that of the gold imbedded in the various sulphides. The questions therefore arise:—

1. Whether the gold is distributed in those ores uniformly or not?

2. Which of the sulphides seem particularly to be the matrix of that metal, and in this case?

3. Which of the respective sulphides is the matrix?

We possess almost nothing but suppositions on this subject. According to Hausmann the gold in the Rammelsberg is probably associated with zinc blende, as this is the case in the Lautenthaler veins;* he concludes this from the fact that in the galena, as well as in the iron and copper pyrites from the Oberharzer veins, which are free from blende, no trace of gold can be discovered. Kerl, however, is of the opinion that because the zinc blendes of the Juliushutte yield the smallest, and the copper ores which are rich in iron pyrites, produce the largest amount of gold, the iron, and perhaps in a still higher degree, the arsenical pyrites, are to be considered as the matrix for the gold, aside of the zinc blende.† In the ores of the Rammelsberg, the copper pyrites are, according to Holzmann, the matrix of this metal.‡ In this country, the copper pyrites are also generally considered as the matrix of a large quantity of the gold occurring in the sulphides. This condition has especially been met in Colorado, where, according to Withney, some beds of this ore often yield over $2,000 per ton. There the Chilean proverb may therefore be regarded as true: "If thou findeth copper, thou hast gold." In North Carolina, Virginia, and elsewhere, the auriferous veins lead, in depth, to copper veins.

* Hausmann, Studien des Gœttinger Vereins bergmænnischer Freunde, Vol. III., p. 332.

† Bruno Kerl, Ergænzungen zur ersten Auflage der Rammelsberger Huttenprocesse, p. 22.

‡ Holzmann's hercyn. Archive, p. 525.

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

By Prof. Henry Morton, Ph. D.

(Continued from p. 69.)

Having considered the evidence which astronomical observation affords as to the source of solar light and heat, we will next pass in review, the proofs we possess concerning those details of structure which we have before described.

The first of these is that supplied by *sun-spots*. Seeing spots in the sun has obtained a proverbial significance, for excess in unamiable criticism, and we might thus suppose that the sun itself is in fact spotless; this is, however, so far from being true, that it is not often possible to find the sun free from spots, or seemingly black markings, when it is viewed through a telescope; while on some occasions, these spots have been sufficiently large to be observed by the unaided eye. To describe these appearances more minutely, we should say that they were irregular dark markings, with a black central portion appearing at one edge of the sun, traveling across his disk in parallel lines having a small inclination to the ecliptic, in the space of about fourteen and a half days, disappearing on the further edge, sometimes finally, at others only to reappear fourteen and a half days after, on the same side as at first, and repeat their journey in the same time as before; in some instances as often as eleven times, consecutively.

As they pass across the solar disk, these spots undergo various changes; in the first place they increase in diameter, or broaden, in the direction of their motion; they also alter their shape in an arbitrary and irregular manner, and besides, occasionally exhibit a rotary movement. Their motion across the disk and widening in that direction, naturally suggests and is explained, by the supposition that they partake the general rotary motion of the sun

(which we thus conclude to be complete in twenty-five days, due allowance for the motion of the earth being made), and owe *this* change in their form to his spherical shape.

As these spots appear on the one side, it is the border of lighter shade which is first seen, and the central spot comes afterwards into view, while as they again disappear on the other edge, it is the border or penumbra, which seems to close over the centre, or umbra, just before the spot disappears. This shows us that these spots cannot be projections, but must be depressions on the solar surface.

It has been observed by De La Rue, Stewart and Lowey, that in a majority of cases, the "following" edge of the spot is more luminous than the surrounding surface; thus seeming to indicate that the luminous matter of the photosphere is especially accumulated on this part, which would be the result had that luminous matter been driven upwards through the space occupied by the spot, from a more central and therefore less rapidly moving region. The spiral shape and individual rotary movement of many spots, suggests to us at once an analogy with our tornadoes, cyclones, or rotary wind storms, and we are thus naturally led to consider the sun-spots as great rents in a canopy of luminous cloud torn asunder, and hurled up and outwards by a rush of heated gas, such as it would be most natural to suppose the heat locally developed by the impact of an infalling meteoric mass, would produce.

The extent and rapid change in shape of these sun-spot, greatly favors the supposition that they are but openings in cloud-like matter; thus sun-spots have been observed, whose area was four times that of the entire surface of our globe, so that in fact four earths in line, arm in arm, might have marched through this breach, without more than *touching* the edge; or regarding this as a cavity, our globe might be at the bottom of it like a bean in a tea-cup. The edges of these spots are found to move at various rates, sometimes as great as 44 miles in an hour. All this is comprehensible in the case of cloud matter, but difficult to apprehend with any less mobile material.

Another class of resemblances between the action of sun-spots and of tornadoes, gives further support to the hypothesis stated above. Tornadoes, as it is well known, are confined chiefly to two zones, north and south of the equator, and similarly we find the sun-spots restricted by boundaries having like relative positions with reference to the solar equator. Now, we believe the development

of tornadoes to be controlled substantially by the excessive heat of the tropics and aerial motions resulting from this cause, combined with the rotary motion of the earth. The motions about the equators of the sun and earth are similar in character, and differ only in degree; the equatorial velocity of the sun being about four times that of the earth. Is there, then, any reason for supposing similarity as regards excess of heat upon the equator? If our hypothesis as to the origin of solar heat may be trusted, this condition is exactly what we should anticipate. The zodiacal light or cloud of infalling matter, lies sensibly in the ecliptic, whose plane makes, with that of the sun's equator, an angle of only 7°. Hence, it is upon the equatorial and not upon the polar regions of the sun that these heat-developing meteors must chiefly fall.

It is true that this does not explain the fact that the sun-spots do not occur upon the equator itself, but only in zones at either side, like those of our trade winds, but neither is this fact inconsistent with the above conclusion, but simply requires some further knowledge for its explanation.

(To be continued.)

LECTURES ON VENTILATION.

By Lewis W. Leeds.

Second Course, delivered before the Franklin Institute, during the winter of 1867-68.

(Continued from page 65).

The phenomena of a burning lamp or candle, is a beautiful emblem of human life. By way of illustrating this part of my subject, I have here a common coal-oil lamp with glass chimney attachment; having lighted which, I will paste a piece of paper over these openings at the base, intended as entrance-passages to the air. The evil result of this operation is visible at once; the flame looks bleared and it smokes most miserably; that lamp, to all appearances, has certainly got the dyspepsia!

The food that feeds that flame is simply undigested for the want of pure air—for the want of oxygen—in other words, for *lack of ventilation.*

Similarly, when you eat your full allowance of food, and do not

breathe sufficient pure air to warm and purify your blood, your whole system becomes *filled* with undigested carbon or "smoke," as we may call it, the same as with that lamp. Likewise the same amount of food will be of less than half its value; just as that oil gives less than half its full light.

One most excellent way to fill your system with smoke, and destroy half your usefulness, is as follows: get up late in the morning, eat a hurried breakfast, and immediately rush to the cars. Sit half or three-quarters of an hour in the close, foul car, and if your feet get thoroughly chilled, all the better for that purpose; a pair of tight boots is the proper thing to prevent the natural circulation of the blood. Have a care to sit with your back to the open window, and your face towards the centre of the foul car; that, too, helps. Your breakfast most probably may not have digested well, and if you feel pretty miserable in about two or three hours, you should commence eating ground-nuts, and keep on at irregular intervals until lunch-time. At this happy period, if you are still a "little under the weather," and not very hungry, go to the restaurant, and eat merely a piece or two of *home-made pie*, with a glass of milk, nothing more! and I should think that would be about sufficient to keep you up to the American standard of the shameful abuse of health. If there should be anything wanting, however, you might accept an invitation to a party, and after spending several hours in a hot, unventilated room, of course you would be too much fatigued to walk home—in fact your thin-soled shoes and light party-dress won't admit of it—and home you ride in a car conditioned as regards wholesomeness and temperature, as aforesaid.

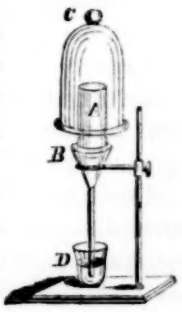
You may not see at once what connection these physiological explanations have with ventilation; but it is the severest tax upon my ingenuity, so to warm and ventilate a room or car, as fully to meet these imperative demands of the physiologist.

Let me say in a few words, always keep your feet warmer than your head, and your back warmer than your face. I will not say now, turn your back to your enemy, but I *will* say, never turn your back to *your best friend*. Eat heartily twice in twenty-four hours (never more than three times), of good, wholesome food, and always after eating sit down for a half or three-quarters of an hour. Remain perfectly quiet, in a room ten degrees hotter than the ordinary temperature of that room, keeping the feet warm, dry and free from pressure, and the back thoroughly protected from draughts,

with a cool fresh breeze blowing in your face. Follow this up carefully, for awhile, and you will not say you cannot afford the time to spend thus, but you will say you cannot afford to *omit* it.

We have here an arrangement by which we wish to explain in a manner the *diffusion of gases;* it has been furnished me by Prof. Albert Leeds, to whom I may say here, I have been much indebted for collateral information and assistance in these lectures.

Fig. 5.



It is often remarked that one gas is a vacuum for another gas. This is hardly the proper way of expressing it, because that would seem to imply that a cubic foot of carbonic acid gas would be diffused into a cubic foot of hydrogen gas, both occupying the one space with the original foot of the latter; but this is not the case. They become mixed, however, by exchanging places. For instance, if we take a glass vessel, just one foot square, containing, consequently, one cubic foot of hydrogen gas, which would weigh thirty-six grains, and place it over a similar vessel, containing one cubic foot of carbonic acid, which would weigh 815 grains, or more than twenty-two times as much as the hydrogen, the heavy carbonic acid gas would rise up into the vessel above, and an equal volume of the light hydrogen gas would fall into the vessel below, so that they would soon become equally diffused.

The apparatus which we have here for illustrating this beautiful phenomenon, consists of a glass tube, with its lower end dipping into a vessel of liquid (crimson-colored, so as to allow its movements to be readily seen), and over the top is placed and sealed in position, an unglazed clay jar. Now, into the glass bell-jar which I hold, mouth downwards, we will allow a supply of hydrogen gas to enter, which, as it is about fifteen times lighter than the air in this room, it will readily do, rising directly to the top, and displacing the air. The vessel being filled, as I judge, I will place it over the unglazed jar, and at once you see the air, as it is expelled from the bottom of the tube, bubbling up through the liquid in the reservoir. This is caused simply by the hydrogen gas rushing through that porous clay cylinder, and displacing an equal amount of the air.

But now we will reverse the operation. We will remove the glass vessel previously filled with hydrogen gas, and then, as the porous cylinder will contain a greater proportion of hydrogen than

the surrounding air, the excess of the gas will rush back again. This result you see palpably and beautifully demonstrated by the rising of the crimson fluid in the tube, and its coursing around the spirals.

Now, this law of the diffusion of gases, is a vitally important one. Did they not possess this property, the different gases would envelope the earth in distinct and separate belts or layers. The poisonous carbonic acid gas being formed in a great miasm near the ground, and being fifteen times heavier than atmospheric air, would cover, primarily, the surface of the earth; then would come a layer of oxygen, next a layer of nitrogen, and so on. Nor would heating them, even supposing the heat should be applied at the bottom, be sufficient to mix them thoroughly, because each density, it would be found, would circulate nearly horizontally by itself.

Hence, it is evident that this becomes an important question with reference to the subject of ventilation; for the reason that, in a still room, although the breath may at first fall to the floor, owing to its having a hundred times as much carbonic acid as ordinary air, yet that would soon be diffused through the whole room. And yet I do not pay so much attention to this, because I think the supply of air ought to be so rapid and abundant, that the slow process of the diffusion of gas would be permitted to exercise but little practical influence.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, December 16th, 1868.

THE meeting was called to order, with the Vice-President, Mr. Coleman Sellers, in the Chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported that at their stated meeting, held December, 9th instant, the resignations of membership in the Board by Messrs. Percival Roberts, William J. Horstmann, and Jacob G. Neafie were accepted.

Also, that donations to the Library were received from the Royal

Geographical Society, the Institute of Actuaries, the Society of Arts, and the Commissioners of Patents, London; the Smithsonian Institution, Washington, D. C.; William B. Thomas, Esq., Burlington, New Jersey, and Joel Giles, Esq., Philadelphia.

The various Standing Committees reported their minutes, and the Special Committee on a revision of the patent laws, reported progress. A paper on the Manufacture of Nitro-Glycerine, communicated by Mr. Stephen Chester, was then read by the Secretary, and the thanks of the Society presented to the author by the Vice-President for the same.

The regular report of the Resident Secretary on Novelties in Science and the Mechanic Arts, was then read, after which some interesting facts in connection with the raising of a portion of the city of Boston, and with the progress of the Suez Canal, were mentioned by Mr. Robert Briggs.

On motion, the Society then went into nomination of officers and members of the Board of Managers for the ensuing year, when the following nominations were made:

President—J. Vaughan Merrick.
Vice-President—B. H. Moore.
Treasurer—Frederick Fraley.
Secretary—Henry Morton.
Auditor—Samuel Mason.

BOARD OF MANAGERS.

John H. Towne,	Geo. P. Roberts,	H. A. Bines,
Washington Jones,	James S. Whitney,	R. H. Lang,
Pliny E. Chase,	James Dougherty,	Wm. B. Wilstack.
Chas. S. Close,	Robert C. Cornelius,	John Birkbeck,
Robert Briggs,	Caleb S. Hallowell,	Chas. Wheeler,
J. Hayes Linville,	Alexander Ervine,	Wm. Helm,
Joseph M. Wilson.		

The Chair then appointed as Judges of Election, Wm. A. Rollin, C. S. Bement, Samuel Hart, Hector Orr, M. W. Haines, Roeper Hoskins, Geo. Gardom.

Mr. Robert Briggs then offered the following resolutions:

Whereas, The rights of ownership of literary productions are based upon the same equitable considerations as those which exist in mechanics' inventions, and both classes of rights possess that individuality which should entitle the author or inventor to a means of protection from the law, well defined and of equal distinctness

to that which the law gives as security to personal property in chattels, or to property in real estate.

And whereas, The complete acknowlegment of the rights of ownership in literary productions, irrespective of nationality, is due to a sense of justice.

Therefore, resolved, That the Franklin Institute, as the representative of the interests of the scientific men, the manufacturers, inventors and mechanics of Philadelphia, expresses this opinion, that it is now desirable that such law, as will establish, define and protect the rights of authors should be enacted by our general Government.

Resolved, 2d. That negotiations and treaties should be formed to the end of obtaining for American authors the acknowledgment of such rights in foreign countries.

Resolved, 3d. That these Resolutions be transmitted to the Senators of our State in the U. S. Senate, to be laid before that body as an expression of opinion by the Institute.

That a Committee of five be appointed to solicit other literary and scientific associations to join in this memorial.

After a brief discussion, it was decided that the consideration of this subject was not within the scope of the Institute, and the motion was therefore laid on the table. The meeting was then on motion adjourned.

HENRY MORTON, *Secretary.*

Sea-Weed Charcoal.—This material, which is prepared from the fine tangle of the Hebrides, is being extensively used in England, as a substitute for animal charcoal, as a filtering medium for water, for deodorizing sewage, clearing white glass, removing acidity from and decolorizing wines and precipitating and decolorizing vegetable alkaloids.—*Chemical News*, p. 49.

A COMPARISON *of some of the Meteorological Phenomena of* DECEMBER, 1868, *with those of* DECEMBER, 1867, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	December, 1868.	December, 1867.	December, for 18 years.
Thermometer—Highest—degree.....	46·00°	52·50°	71·00°
" date.....	21st.	27th.	2d, '59.
Warmest day—mean ..	41·33	47·33	62·80
" " date.....	21st.	28th.	2d, '59.
Lowest—degree.....	13·00	8·00	4·50
" date.....	25th.	12th & 14th.	19th, '56.
Coldest day—mean	20·33	14·17	11·00
" " date	24th.	13th.	18th, '56.
Mean daily oscillation...	9·18	11·84	11·93
" " range.....	4·22	6·16	6·35
Means at 7 A. M.	30·72	29·26	31·78
" 2 P. M.	34·56	34·13	38·64
" 9 P. M.	32·36	31·77	34·49
" for the month....	32·55	31·72	34·97
Barometer—Highest—inches.....	30·489	30·558	30·678
" date.....	13th.	19th.	18th, 56.
Greatest mean daily pressure	30·415	30·516	30·611
" " " date...	13th.	19th.	18th, '56.
Lowest—inches	29·398	29·548	28·946
" date.....	17th.	6th.	9th, '55.
Least mean daily pressure...	29·520	29·704	29·175
" " " date...	8th.	17th.	8th, '54.
Mean daily range.....	0·239	0·256	0·219
Means at 7 A. M	30·042	30·038	29·960
" 2 P. M	30·027	30·027	29·920
" 9 P. M	30·052	30·032	29·948
" for the month	30·040	30·032	29·943
Force of Vapor—Greatest—inches	0·240	0·361	0·551
" date	20th.	27th.	2d, '59.
Least—inches.....	·051	0·55	·025
" date.....	24th.	13th.	18th, '56.
Means at 7 A. M.....	·123	·133	·144
" 2 P. M.....	·124	·131	·162
" 9 P. M.....	·139	·146	·156
" for the month...	·129	·137	·154
Relative Humidity—Greatest—per cent	90·0	93·0	100·0
" date.....	Often.	12th & 27th	Often.
Least—per cent....	38·0	41·0	23·0
" date.....	10th & 19th	29th.	15th, '61.
Means at 7 A. M....	69·6 per ct	78·5 per ct	77·1 per ct
" 2 P. M....	60·2	65·1	64·9
" 9 P. M....	73·4	77·1	75·2
" for the month	67·7	73·6	72·4
Clouds—Number of clear days*.	8·	5·	8·6
" cloudy days	23·	26·	22·4
Means of sky covered at 7 A. M	62·3 per ct	72·9 per ct	64·0 per ct
" " " 2 P. M	65·5	70·6	63·8
" " " 9 P. M	51·6	52·6	49·1
" " for the month	59·8	65·4	59·0
Rain and Melted Snow—inches.....	4·27	2·860	3·793
No. of days on which rain or snow fell .	8·	·12	10·5
Prevailing Winds—Times in 1000.	s54°13′w.327	s52°46′w.201	s62°46′w.272

* Sky one-third or less covered at the hours of observation

A COMPARISON *of some of the Meteorological Phenomena of the year* 1868, *with those of* 1867, *and of the last* SEVENTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11¼′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	1868.	1867.	17 years.
Thermometer—Highest—degree	98·00°	91·00°	101·00°
" date	July 14.	July 4.	July 17, '66.
Warmest day—mean	90·50	85·67	92·33
" " date	July 14.	July 4.	July 17, '66.
Lowest—degree	3·00	8·00	—9·00
" date	Feb. 23.	Dec. 12 & 14.	Jan. 8, '66.
Coldest day—mean	10·83	14·17	—1·00
" " date	March 3.	Dec. 13.	Jan. 9, '56.
Mean daily oscillation	13·35	14·48	14·71
" " range	5·29	5·30	5·52
Means at 7 A. M.	48·87	49·27	49·88
" 2 P. M.	57·31	58·17	59·67
" 9 P. M.	52·29	52·78	53·25
" for the year	52·82	53·41	54·27
Barometer—Highest—inches	30·748	30·970	30·970
" date	Feb. 1.	Feb. 11.	Feb. 11, '67.
Greatest mean daily pressure	30·672	30·862	30·862
" " " date	Feb. 23.	Feb. 11.	Feb. 11, '67.
Lowest—inches	29·115	28·778	28·778
" date	March 2.	May 8.	May 8, '67.
Least mean daily pressure	29·249	29·013	28·958
" " " date	March 2.	May 8.	Ap'l 21, '52.
Mean daily range	0·185	0·187	0·159
Means at 7 A. M.	30·028	29·996	29·892
" 2 P. M.	29.995	29·962	29·853
" 9 P. M.	30·012	29· 977	29·879
" for the year	30·012	29·978	29·875
Force of Vapor—Greatest—inches	0·911	0·925	1·059
" date	July 12.	July 6.	June 30, '55.
Least—inches	·038	·042	.013
" date	Feb. 23.	Jan. 30.	Feb. 6, '55.
Means at 7 A. M.	·321	·316	·324
" 2 P. M.	·347	·325	·339
" 9 P. M.	·349	·341	·346
" for the year	·337	·327	·336
Relative Humidity—Greatest—per cent.	100·0	100·0	100·0
" date	March 21.	Oct. 29.	Often.
Least—per cent.	28·0	21·0	13·0
" date	Oct. 17.	April 6.	Ap'l 13, '52.
Means at 7 A. M.	75·5	75·6	75·5
" 2 P. M.	59·8	57·7	57·3
" 9 P. M.	73·5	71·9	72·1
" for the year	69 6	68·4	68·3
Clouds—Number of clear days*	82·	108·	108·
" cloudy days	284·	257·	257·
Means of sky covered at 7 A. M.	66·6 p. c.	62·9 p. c.	60 5 p. c.
" " " 2 P. M.	63·8	61·9	61·1
" " " 9 P. M.	54·0	48·6	46·5
" " " for the year	61·5	57·8	56·0
Rain and melted snow—Amount—inches	50·180	62·935	47·117
No. of days on which rain or snow fell	128·	134·	127·
Prevailing Winds—Times in 1000	N74°58′W.194	N71°34′W.152	N74°58′W.194

* Sky one-third or less covered at the hours of observation.

A General Abstract of the Meteorological Observations made at Philadelphia during the year 1868. By JAMES A. KIRKPATRICK, A.M.
Latitude 39° 57½′ N. Longitude 75° 11¼′ W. from Greenwich. Height of Barometer fount, sixty feet above mean tide in the Delaware River.

1868. MONTHS	Thermometer.									Barometer.								Dew Point.			
	Maximum.	Minimum.	Range. Monthly.	Range. Mean daily.	Mean daily oscillation.	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Means. Average.	Highest.	Lowest.	Range. Monthly.	Range. Mean daily.	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Means. Average.	Means. 7 A. M.	Means. 2 P. M.	Means. 9 P. M.	Means. Average.
	°	°	°	°	°	°	°	°	°	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	°	°	°	°
January.....	46	11	35	5·80	11·79	27·84	33·23	29·87	30·31	30·580	29·291	1·289	·271	30·010	29·986	30·028	30·008	22·64	22·21	23·88	22·91
February....	52	3	49	7·84	16·00	21·72	31·21	26 72	26·55	·748	·566	1·182	·278	30·197	30·154	30·156	30·169	16·58	21·53	21·28	19·80
March..	76	6	70	6 97	15·68	35·55	46·34	40·69	40·86	·600	·115	1·485	·258	30·041	30·021	30·032	30·031	28 54	31·73	32 28	30·85
April.....	70	23	47	7·73	16 80	42·28	52·78	46·38	47·15	·458	·192	1·266	·241	30·035	29 980	29·989	30·001	33·54	35·92	37·41	35·62
May.....	77	40	37	4·65	14·80	54.32	63·50	57·19	58·34	·269	·375	·834	·144	29·846	29 835	29·841	29·841	47·53	50·08	50·71	49·44
June.....	90	51	39	4·10	15·97	67·12	77·85	70 85	71·94	·292	·629	·663	·119	30·000	29·970	29·978	29·983	58·38	60·30	61.09	59·93
July.....	98	65	33	3·13	13·32	77·26	86·60	80 79	81·55	·163	·623	·540	·064	29·962	29·928	29·942	29·944	68·70	69·74	70·52	69·65
August.....	89	61	28	2·87	10 65	74·26	80·90	77·37	77·51	·279	·627	·652	·102	30·006	29·976	29·986	29·989	64·73	64·84	65·80	65·12
September...	88	48	40	5·70	11·83	64·10	72·18	67·37	67·88	·441	·764	·677	·123	30·046	29·997	30·021	30·022	55·64	57·25	58·03	56·97
October	70	34	36	5·12	12·06	48·92	58·24	52·92	53·36	·552	·685	·867	·190	30·144	30·105	30·122	30·124	41·84	43·47	43·90	43·07
November...	72	33	39	5·37	12·10	42·38	50 30	45·02	45·90	·418	·355	1·063	·187	30,065	29·963	29·992	29·987	34·99	33·26	35·74	34·66
December ...	46	13	33	4·22	9·18	30·72	34·56	32·36	32·55	·489	·398	1·091	·239	30·042	30·027	30 052	30·040	22.10	21·89	24·67	22·88
Annual means, ...	98	3	95	5·29	13·35	48·87	57·31	52·29	52 82	30·748	29·115	1·633	·185	30·028	29·995	33·012	30·012	41·27	42·68	43·78	42·58
Winter.....	52½	3	49½	6·60	13·31	26·27	32·86	29·45	29·53	30·748	29·291	1·457	·268	30·082	30·056	30·072	30·070	20 85	22·36	23·53	22·25
Spring.....	77	6	71	6·45	15·76	44·05	54·21	48·09	48·78	·600	·115	1·485	·214	29·974	29 945	29·954	29·958	36·54	39·24	40·13	38·64
Summer.....	98	51	47	3·37	13·31	72 88	81·78	76·34	77·00	·292	·623	·669	·095	29·989	29·958	29·969	29·972	63·94	64·96	65 80	64 90
Autumn.....	88	33	55	5·40	12·00	51·80	60·24	55·10	55·71	·552	·355	1·197	·167	30·065	30·022	30·045	30·044	44·16	44·66	45·89	44·90
Means for 17 years.	101	—9	110	5·52	14·71	49·88	59·67	53·25	54·27	30·970	28·778	2·192	·159	29·892	29·85[illegible]	29·879	29·875		43·26		